

Controversies Within the Scientific Revolution

*Edited by Marcelo Dascal
and Victor D. Boantza*

CONTROVERSIES
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Controversies Within the Scientific Revolution

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Volume 11

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Table of contents

Introduction: Controversies and the dialectical texture of the Scientific Revolution <i>Marcelo Dascal and Victor D. Boantza</i>	1
Part I. Astronomy and mechanics	
Honoré Fabri S. J. and Galileo's law of fall: What kind of controversy? <i>Michael Elazar and Rivka Feldhay</i>	13
Galileo, the Jesuits, and the controversy over the comets: What was <i>The Assayer</i> really about? <i>Ofer Gal and Raz Chen-Morris</i>	33
Fair-mindedness versus sophistry in the Galileo affair: Two controversies for the price of one <i>Maurice A. Finocchiaro</i>	53
Part II. Light and gravity	
From cohesion to <i>pesanteur</i> : The origins of the 1669 debate on the causes of gravity <i>Victor D. Boantza</i>	77
Leibniz versus Newton on the nature of gravity and planetary motion <i>Nir Grannot</i>	101
The argumentative use of methodology: Lessons from a controversy following Newton's first optical paper <i>Gábor Á. Zemplén</i>	123

Part III. Physiology and vitalism

Salient theories in the fossil debate in the early Royal Society: The influence of Johann Van Helmont <i>Anna Marie Roos</i>	151
Were the arguments of William Harvey convincing to his contemporaries? <i>Adelino Cattani</i>	171
Why was there no controversy over Life in the Scientific Revolution? <i>Charles T. Wolfe</i>	187

Part IV. Human sciences and theology

The pre-Adamite controversy and the problem of racial difference in seventeenth-century natural philosophy <i>Justin E. H. Smith</i>	223
Scientific revolution in the moral sciences: The controversy between Samuel Pufendorf and the Lutheran theologians in the late seventeenth century <i>Merio Scattola</i>	251
Contributors	277
Index	283

INTRODUCTION

Controversies and the dialectical texture of the Scientific Revolution

This collection explores the rich texture of debates that comprised the so-called Scientific Revolution, from which the modern conception of science – the “new science” or the “new philosophy” as it was sometimes referred to in the seventeenth century – had emerged. For Whitehead it was the core of what he called “the century of genius”. Countless studies have been dedicated to this major development, which led to many of its details, and to its revolutionary character relative to the preceding conceptions and practices of science. Much detailed scholarly attention has been paid to the ‘controversial’ character of this transformative episode, to the extent that the very notion of the ‘Scientific Revolution’ – as opposed to the generic designation ‘a revolution in science’ – has recently turned into a polemical lightning rod among historians and philosophers of early modern science and technology.

The boldest articulation of these tensions is offered by Steven Shapin, whose book *The Scientific Revolution* opens with the declaration: “There was no such thing as the Scientific Revolution, and this is a book about it” (Shapin 1996: 1). The use of ‘revolution’ to depict the birth of modern science goes back to the Enlightenment *lumières*, the admirers and self-proclaimed intellectual heirs of Bacon, Locke, and Newton. The marquis de Condorcet (1743–1794) was probably the originator of the term *révolutionnaire* and its application in this context (Cohen 1985: 224), thus paving the way for a vast tradition of debate and progressive elaboration of the notion, which runs through the works of Kant, William Whewell, Pierre Duhem, Edwin Burt, Alexandre Koyré, and Thomas Kuhn (Floris Cohen 1994), among others. The historiographical controversy over the scientific revolution – its chronology, topography, subjects, heroes, results, legacy, and unity – has never abated, as we still witness attempts to “rethink” (Osler 2000) and “reappraise” its character (Lindberg and Westman 1990), view it within a “national context” (Porter and Teich 1992) as a characteristic of the *République des Lettres* (Dascal and Marras 2002), or once more question its revolutionary nature (Barker and Ariew 1991).

Yet as much as there was, and still is, controversy *around* the scientific revolution, there was controversy *within* it. Controversies and other kinds of

polemical exchanges – whether brief or prolonged, focused or diffused, regional or multinational, analytic or synthetic – formed an integral and fundamental component of the complex historical, social, and cultural shifts which have forever transformed our understanding and view of the natural world sometime during the century spanning Kepler's *Astronomia nova* (1609) and Newton's *Opticks* (1704). Recognizing the role of controversy within these processes is therefore essential for establishing a deeper and richer interpretation of the scientific revolution.

From the beginning of the scientific revolution around the late sixteenth century to its final crystallization in the early eighteenth century, hardly an observational result, an experimental technique, an explanation, a theory, a mathematical proof, a methodological principle, or the award of recognition and reputation remained unquestioned for long. Competing alternatives to any and all of these ideas, practices, and discoveries, arose as soon as the proposals appeared, and ceaseless debate surrounded each step ahead. Historians, sociologists, and philosophers of science and of early modern culture, who have spent much effort towards understanding the scientific revolution, ought to complete the picture by inquiring about both the details and the broad significance of the role of controversy therein. Drawing upon the recent proliferation of micro-studies on the one hand, and the problematization of the standard conceptual categories usually employed on the other, this collection addresses this issue through a series of studies focusing on the background of the intense disputes, controversies, and polemics against which modern science has emerged.

Was unbridled criticism and debate a mere social or psychological by-product of the liberation from the authority of a previous conception of science and of religious, intellectual, political, and social constraints? Were controversies marginal episodes, restricted to certain fields, or were they the rule in the majority of scientific domains? To what extent did the scientific controversies share a typical pattern which distinguished them from debates in other fields? After all, wasn't the much praised *République des Lettres* that emerged at the time characterized by the omnipresence and intensity of its quarrels (Dascal and Marras 2002), which ultimately led to a new approach to nature, knowledge, and rationality? What do controversies within the scientific revolution teach us about the ways through which new knowledge and standards of objectivity were being produced, as part of a continuous negotiation between the old and the new? It is with such questions in mind that we have put this volume together. Answers to these encompassing historical and philosophical questions are sought through a close attention to specific controversies within and across the changing scientific disciplines as well as across the borders of the natural and the human sciences, philosophy, theology, and technology.

The chapters examine a wide array of debates, considering both historical and historiographical issues. They explore a range of themes, figures, and locales, some better known than others: from Galileo's telescopic observations or Newton's mathematical achievements to lesser known natural philosophers, theologians, jurists, and early modern experimenters, all of whom shared the goal of understanding the natural world order and its social counterpart through various processes of conceptual and material transformations. The studies assembled here examine theoretical and moral problems, particular experiments, observations, discoveries, inventions, and methodological innovations, as they evolved through the intervention of several disputants. Some chapters focus on one major debate, or reveal a previously unknown dispute; others are concerned with clusters of interrelated controversies, following their evolution and analyzing the interference of philosophical and/or theological considerations in their dialectical structure, argumentation, and dynamics.

The chronological span covered by this volume stretches from the early Copernicans of the last third of the sixteenth century to the Newtonians of the first third of the eighteenth century. Despite differences in time, methodology, and subject matter, all the chapters are concerned with determining – whether by evidencing, illustrating, questioning, problematizing, and various combinations thereof – the constitutive role of controversies within the broader network of intellectual and material changes that comprised the scientific revolution. Likewise, the volume seeks to contribute to our understanding of the notion of controversy by focusing on the scientific revolution as a particular and unique historical episode and process.

The readers will certainly notice the variety of the meanings attributed to the term 'controversy' in different chapters; they may also conjecture that this is due to the fact that we, qua editors, did not try to impose a single definition of this term. In fact, we hoped to highlight thereby the multiple forms of debate that make the pursuit of knowledge so dialectic and dynamic, even within the emerging scientific endeavor to establish systematic epistemic procedures (see, e.g., Gloning 2002). Needless to say that the largely different forms of polemic exchanges identifiable in the debates analyzed in each chapter require a theoretical distinction and classification in order to become useful analytic tools, capable of helping to solve the 'scientific revolution' debate or puzzle described above. It is not our intention to propose here such a theory. We would like, however, to point out that our selection of 'controversy' for the title of this volume is not casual. In fact, it alludes to the use of this term as a technical designation (henceforth italicized) of a specific type of debate, in one of the extant classifications of debates. In this sense, *controversy* is distinguished from *discussion* and from *dispute*, two other specific types of debate (see, e.g., Dascal 1998a, 1998b, 1998c). Among its

features, a *controversy*, unlike its counterparts just mentioned, does not rely on an established decision procedure that determines which of the two positions in conflict in a debate is the correct one and thereby excludes the other. Therefore, its basic structure is not dichotomous and it allows for the eventual conciliation of opposite views in the construction of a new theory and even for a new methodology. It thus contributes to the development of knowledge by paving the way for innovation. In this respect, the presence or absence of *controversies* in a field may be seen as an indicator of its evolution or, if you wish, of its 'revolutionary' status or stage – as some of the chapters indeed suggest. While this structural feature of *controversy* may account for a significant part of the perception of the 'scientific revolution' as a unified whole, it resists the temptation of reducing such a unity to a single set of basic concepts, principles, or methods, preserving its variegated nature, in spite of their shared concern with understanding the natural world and advancing our knowledge thereof.

The eleven essays in *Controversies Within the Scientific Revolution* represent new work by an international group of historians, philosophers, and historians of science. Some have their origin in an International Workshop organized by Adelino Cattani of Padua University with the cooperation of the International Association for the Study of Controversies (IASC), which was held in Padua in May 2008. Others have been commissioned and enhance the overall historical and philosophical balance of the book. As the essays attest, an examination of the nature and role of controversies in this formative period in the history of western thought sheds light on the complex ways in which disputes and disagreements served to create and consolidate knowledge – theoretical and empirical – about the natural world and the newly created spaces for humans within it. The volume follows four distinct yet interlinked themes: (i) astronomy and mechanics; (ii) physiology and vitalism; (iii) light and gravity; and (iv) the human sciences and theology.

Michael Elazar and Rivka Fledhay analyze the Jesuit Honoré Fabri's (1607–1688) critical position towards Galileo's theory of free fall. Challenging received distinctions between Galileans and anti-Galileans the chapter shows that Fabri's position was only one step in a dialectical move that aimed to incorporate many of Galileo's theories and results into the contemporary Jesuit physico-mathematical discourse. The chapter argues for a broad concept of controversy by privileging commonalities within historical and philosophical rivalries, with particular focus on such shared grounds within the Jesuit-Galilean dispute, which in turn questions the Jesuits' common depiction as uniform supporters of Aristotelianism. Whereas Galileans (as 'moderns') tended to overemphasize the ancients-versus-moderns dichotomy, Elazar and Feldhay argue, the Jesuits perceived themselves as capable of providing an overall perspective by negotiating and mediating

various – ancient, medieval, and contemporary – views and incorporating them within an Aristotelian-Archimedean framework.

Ofer Gal and Raz Chen-Morris recast one of the single most influential controversial manifestos of the scientific revolution – Galileo’s *Assayer* of 1623 – in a radically new light. They do so by setting it against the backdrop of Galileo’s preceding debate with the Jesuit Horatio Grassi, over the physical and astronomical import of comets. On this novel reading, *The Assayer* is shown to be neither an introduction of an “open system of scientific inquiry in opposition to the closed system of the schools”, as Stillman Drake immortalized it (Drake and O’Malley 1960:xxiii), nor a demonstration of Galileo’s mathematical Platonism, as suggested by Alexandre Koyré (1943). Nor was it part of Galileo’s struggle to free Copernicanism from the dictates of scriptural interpretation. Gal and Morris-Chen demonstrate that it was rather part and parcel of a “wholly empirical argument – the most curious attempt to discredit the value of parallax observations”. This argument is then traced to the conflicting conceptions of vision, directness, mediation, and the relative status of the eye and the optical instrument, as shared by the ‘ancient’ Jesuits and the ‘modern’ Galileo, respectively.

Maurice Finocchiaro explores the interrelationship between two controversies, one historical and one historiographical: Galileo against the Church on Copernicanism and divergent accounts of Galileo’s “fair-mindedness” principle. The latter was first picked up by the Paduan Antonio Querenghi (1546–1633), and later discussed by modern scholars like Ludwig von Pastor (1854–1928), Adolf Müller (1853–1939; see Müller 1909), and Arthur Koestler (1959). Finocchiaro provides a close examination of this method of argumentation, restoring its lost historical, methodological, and rhetorical luster. He then proceeds to implement it in his own analysis of the two controversies, advancing thereby an alternative understanding of what has come to be known as the Galileo Affair (which ended with Galileo’s condemnation by the Inquisition). Following Querenghi’s cue, Finocchiaro depicts the “fair-mindedness” method as a potent Galilean technique consisting in strengthening objections before refuting them, thus depicting the objections of interlocutors as serious, important, and plausible, namely, rendering his rivals as utterly reasonable and rational actors. The chapter envisages modern commentators’ misinterpretation and dismissal of this method as a meta-controversy – a controversy about how to interpret the Galileo Affair and Galileo’s methodological conduct therein.

Victor D. Boantzsa establishes and analyzes the connections between two controversies: the 1667 debate on the nature of gravity at the French Royal Academy of Sciences, and a lesser known series of ‘smaller’ interlinked disputes about the nature of cohesion and solidity, which took place in the same Academy earlier in the same year. The analysis demonstrates the links between the two controversies,

showing how various concerns expressed in the debate on the nature of the ‘forces’ acting at the level of macro-matter (gravity) were influenced by the preceding arguments about the characteristics of material interactions at the level of micro-matter (cohesion). Boantz’s analysis thus illustrates the interrelationship between mechanical and non-mechanical interpretations of attraction, and theoretical and empirical agendas informed by vitalistic and alchemical perceptions of matter, activity, and material change.

Nir Grannot analyzes the prolonged controversy between Newton and Leibniz on the nature of gravity and the cause of planetary motion, which followed in the wake of Newton’s *Principa mathematica* (1687) and Leibniz’s (1689) *Tentamen de motuum coelestium causis* (essay on the causes of celestial motions). Grannot’s account describes the relations between the various metaphysical, epistemological, and theological considerations, which fell under the compass of this rich debate, and the rival argumentative strategies employed by the protagonists to present and defend their competing outlooks. By revealing the architecture of the controversy, Grannot throws new light on traditional (historical and historiographic) dichotomies such as rational vs. empirical, natural vs. miraculous (or unnatural), intelligible vs. occult, or material vs. spiritual, while delineating their respective roles within the divergent cosmologies of Newton and Leibniz. In the final analysis, Grannot points to the meta-scientific character of the controversy, surrounding “questions such as what kind of hypothesis should be admitted as a possible explanation; what is the role (if any) of metaphysical principles in a scientific theory; [and] what are the limits of the inductive method of reasoning”. The answers to these questions were directly informed by the corresponding views of the interlocutors concerning the relations between Nature and Reason.

Gábor Á. Zemplén explores the methodological tensions arising from attempts to reconstruct one of the first major debates of the scientific revolution to occur in a journal, which was prompted by the publication of Newton’s first paper in the *Philosophical Transactions* on a “New Theory of Light and Colours”. Zemplén probes various reconstructions of ‘methodology’ (as both concept and interpretive procedure) in argumentative situations vis-à-vis the positions adopted by the interpreters with respect to the protagonists within a controversy; the extent to which such positions are shaped by the argumentative situation; and the viability of what he terms the “radical dialectification” of controversies, a charitable “process of reconstruction that renders positions much more context-sensitive ... than socializing approaches to history of science usually do”. By addressing such issues, and by examining the interplay between argumentative and methodological historiographical interpretations of disputes, Zemplén argues for the distinctive role of argumentative situations in shaping the protagonists’ positions and subsequently in the development of modern science.

Anna Marie Roos examines a series of debates about the origins of fossils, which arose among members of the early London Royal Society, and which largely revolved around the question whether fossils were remnants of past animal and plant life forms (alluding to the heretical possibility of extinction) or were created spontaneously by nature. Roos focuses in particular on Martin Lister's and Robert Plot's (1640–1696) explanations of such 'generative powers' and 'seeds', showing how they actually comprised differing adaptations of or reactions to the Belgian physician Johann Van Helmont's (1579–1644) theories in saline chymistry. By pointing to the influence of Van Helmont's chymical ideas – about ferments, spirits, solvents, and other immaterial-spiritual agents and forces – on leading members of the early Royal Society, commonly portrayed as avowed empiricists, experimentalists, and prototypical exemplars of the New Science, Roos illustrates the transformation, dislocation, and reabsorption of contemporary controversial notions, through dialectical patterns of inclusion and exclusion within the conceptual spheres of the newly forged knowledge.

Adelino Cattani reinterprets the reception of William Harvey's (1578–1657) *Exercitatio Anatomica De Motu Cordis et Sanguinis in Animalibus* by his contemporaries. Published in 1628, *De Motu Cordis* has long since been considered as one of the seminal texts of the scientific revolution in which Harvey established the circulation of the blood on an empirical and quantitative basis. Challenging this traditional reading, Cattani reconstructs Harvey's discovery, invention, and justification of his key arguments from the perspective of a controversy, in which notions such as demonstration, proof, and even evidence evolve dynamically and acquire their epistemological standing through dialectical processes of negotiation. In this context, Cattani's account offers a reconsideration of the ways by which Harvey deployed various strategies of argumentation, demonstration, justification, and persuasion, to impose his novel theory of circulation "within an exceptionally short time and in spite of very strong opposition".

Charles Wolfe approaches our subject from a different standpoint, by asking why there was no controversy over Life in the scientific revolution. Wolfe's analysis demonstrates how before the invention of the term 'biology' in the early eighteenth century, the question of Life – the status of living organisms within the broader physico-mechanical universe – agitated different corners of the European intellectual scene. From modern Epicureanism to medical Newtonianism, and from Stahlian animism to the discourse on animal economy in vitalist medicine, models of living beings were constructed in opposition to merely anatomical, structural, and mechanical models. Yet in the iconic definitions and accounts of the scientific revolution, from their early formulations onwards, we witness a conspicuous absence of worry over what status to grant living beings in the newly born physicalized universe. Neither Harvey, nor Boyle, nor Locke ever ask

directly what does or does not separate organisms from other entities. Wolfe's account shows that even though 'Life' was a major source of contention and debate in early modern thought, the scientific revolution missed the controversy.

Justin E. H. Smith explores the gap between early modern conceptualizations of human variety and theories of race that emerged in the course of the nineteenth century, tracing the difference to two distinct but related ways of accounting for the unity and the distinctness of ethnic or 'racial' groups. Whereas "autochthony" centers on the descent from common parents, by what Kant would later call "the unity of the generative principle", "traduction" identifies shared membership in a discrete population as stemming from a connection to a certain territory of the earth. Smith's argument shows that traductionism dominated early modern accounts of human diffusion throughout the globe, coupled with the view that over time climates and environments change the physical appearance of the human populations that inhabit them, even though no amount of change in appearance could ever lead to change in species. Focusing on pre-Adamism, the one prominent instance of a non-traductionist explanation of human origins in the seventeenth century, Smith demonstrates that it was not motivated by a preference for autochthony, but rather by a concern to establish the legitimacy of parallel cultural traditions, especially in the sciences of chronology and astronomy that are not mentioned in Judeo-Christian Scripture. Smith makes the case for this cluster of proposals by examining closely the work of two pairs of seventeenth-century authors – Isaac La Peyrère (1596–1676) and Matthew Hale (1609–1676), and François Bernier (1625–1688) and Leibniz – who were at the center of early modern controversies concerning the origins and diversity of the human species.

Merio Scattola examines the rise of natural law and moral science as an academic discipline as a typical product of the late seventeenth century, drawing parallels between it and the history of the natural sciences during the time of the scientific revolution. Scattola follows natural law from its introduction into academia in 1663, when Karl Ludwig, prince elector of the Palatinate, appointed Samuel Pufendorf (1632–1694) as professor of natural law at the university of Heidelberg. Pufendorf was subsequently embroiled in a controversy about the epistemological foundations of his doctrine that lasted over two decades, and in which Swedish and German theologians and philosophers opposed his ideas and insisted on a more orthodox foundation of moral philosophy upon moral theology. Analyzing and contextualizing this controversy, and arguing that it corresponds to a 'revolution' in the moral sciences, Scattola demonstrates its importance for understanding the emergence of modern moral philosophy.

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PART I

Astronomy and mechanics

Honoré Fabri S. J. and Galileo's law of fall

What kind of controversy?

Michael Elazar and Rivka Feldhay

1. Introduction

Our paper focuses on one episode within the broader dispute over the philosophy of motion between Jesuit mathematicians and a group of natural philosophers often identified in the historical literature as 'mechanical philosophers'.

Galileo's greatest achievement – the formulation and experimental verification of a mathematical law of fall – had prepared the setting which gave rise to competing subsequent theories of free fall, including the Jesuit Honoré Fabri's version, which occupies a considerable portion of his *Tractatus physicus de motu Locali* (1646). In what follows we reconstruct the crystallization of Fabri's position on free fall out of his critique not only of Galileo and his followers, but also of the Cartesians and other contemporary 'mechanistic philosophers' on the one hand, and of the Aristotelian tradition on the other.

Our basic thesis is that Fabri's critical position towards Galileo's theory of free fall – which he shared with other Jesuits – was only one step in a dialectical move that aimed to incorporate into the Jesuits' physico-mathematical discourse many of Galileo's theories and results. A close inspection of Fabri's position illustrates the inadequacy of the commonly accepted dichotomy between Galileo's *amici* and *nemici* (or between *Galileisti* and *oppositori*), and will further reveal the need for a broad concept of controversy, of which the following definition, by Gideon Freudenthal (2002: 573), could be regarded as an example: "Controversy" is here understood as referring to an ongoing antagonistic exchange over a disagreement that cannot be readily resolved by the means at hand. However, the issue is being discussed because the participants believe that the controversy will be resolvable in the framework of a more advanced view which will be generated by the dispute". Although here we do not analyze a concrete controversy, we clearly show that in order to fully understand the elaborate cultural field which emerged around Galileo and his activities (see Feldhay 1999), it is necessary to abandon a

dichotomous notion of conflict between theories and/or their followers and employ instead a more sophisticated concept of ‘controversy’, implying an extensive common background that renders possible the abovementioned dialectics.

As we will show, the Jesuit-Galilean dispute cannot be reduced to its controversial aspects alone. The institutional constraints that worked against the incorporation of Galilean science in a straightforward way in the Jesuit educational system will also be pointed out. Thus, the significance of the dispute in the eyes of contemporaries, and especially its reproduction in modern historiography of science will be clarified.

The historical background to understanding Fabri’s position has been described by Paolo Galluzzi (2001), in his article “Gassendi and l’affaire Galilée”. Pierre Gassendi, in his *De motu impresso a motore translato* (1642), was the first to comment on Galileo’s theory and to clearly lay out the fundamentals of these intensive discussions, referred to by Galluzzi as “the second Galilean affair” (the first one being, of course, the sequence of events which ended in Galileo’s 1633 condemnation). This “second Galilean affair” involved, among others: in France – Mersenne, the ‘coordinator’ of this lively correspondence, the Jesuits Fabri and Le Cazre, Roberval, Le Tenneur and, more marginally, Descartes and Fermat; in Holland – the (very) young Christian Huygens; in Italy – Torricelli, Ricci, and Baliani (Galluzzi 2001: 239–240).

Fabri’s complex attitude towards Galileo’s heritage may be discerned in the following passage, taken from the beginning of the preface to his *Tractatus physicus*:

Many have fruitfully exerted themselves until now in this matter. Indeed the Great Galileo, who before any one else wonderfully and with almost divine sharpness of genius led local motion to where no mortal had led it before; nevertheless because he omitted many things which relate to motion, as everyone knows, and did not prove those marvelous effects from physical principles, but only assented to some proportions from geometrical [principles]; in order to have regard for physics, we undertake another way: we do consult geometry, to explain and set forth those aforementioned proportions which belong to motions; but we reduce the effects connected to those proportions to physical principles. (Fabri 1646: 5)¹

1. “Multi sane hactenus in hac materia feliciter desudarunt; & quidem prae caeteris magnus ille Galileus, qui mirifica, & fere divina ingeni iacie, motum localem eo perduxit, quo mortaliū nemo perduxerat; quia tamen multa omisit, quae ad motum spectant, ut nemo nescit; nec ex principiis Physicis mirabiles illos effectus demonstravit, sed tantum certis quibusdam proportionibus ex geometricis addixit; ut Physicae consulamus, aliam inimus viam: Geometriam quidem adhibemus, ad explicandas, exponendasque praedictas illas proportionēs, quae motibus insunt; sed effectus illos praedictis proportionibus affixos ad principia Physica reducimus”. Unless otherwise stated, all the translations are ours.

Summarizing here the achievements and shortcomings of the “Great Galileo”, Fabri refers first and foremost to Galileo’s ‘mathematical’ theory of free fall. Galileo’s most substantial contribution to the science of motion – which, in Fabri’s words, “led local motion to where no mortal had led it before” – was asserting the simple (and correct) linear proportionality between velocity and time ($v \propto t$) and deriving (and ultimately experimentally verifying) the ‘law of odd numbers’, which states that the spaces traversed in equal times increase according to the series 1, 3, 5, 7, etc. (Galilei 1989: 153–154, 167–169).

However, within his mathematical examination of free fall Galileo saw no point in entering “into the investigation of the cause of the acceleration of natural motion”, and for him it sufficed “to investigate and demonstrate some attributes of a motion so accelerated” (ibid.: 159).² Fabri opposed this approach and set out to discover and formulate the cause of accelerated natural motion. In addition, Galileo’s mathematical approach – anchored in the claim that any continuum is composed of actually infinite mathematical indivisibles, which implied infinite degrees of velocity between 0 and any finite velocity (ibid.: 167–157) – was likewise rejected by Fabri. Instead of Galileo’s ‘continuous’ approach, he adopted a discrete analysis, which does not entail ‘actual’ infinities; according to his general conception of motion he ascribed the cause of acceleration of falling bodies to impetus. These were the parameters within which Fabri’s approach evolved, as will be described in the following pages.

2. The emergence of Fabri’s theory of free fall

2.1 Reframing the Aristotelian discourse on free fall

The second book of the *Tractatus physicus de motu locali*, entitled *De motu naturali deorsum*, starts by establishing impetus – defined in the first book as the quality which causes motion in general (Fabri 1646: 2)³ – as specifically the cause of accelerating free fall, while criticizing the explanation of Aristotle, which had been warmly embraced by the scholastic tradition. The reframing of the Aristotelian discourse is achieved through establishing two central principles: that “natural

2. It is worth noting that after the publication of his *Two New Sciences* in 1638, Galileo requested that a demonstration of the basic postulate from which he derived the law of fall should be added to all subsequent editions. This may indicate his reluctance to give up altogether the search for causes of acceleration. See Galilei (1989: 162, n. 19, 171–175).

3. “Definitio III: impetus est qualitas exigens motum, seu fluxum localem sui subiecti”.

local motion” is downward motion (and downward *only*), and that it is generated by an *inner* cause, not an external one.

At first sight all this looks quite Aristotelian. Fabri uses the language of “natural motion” which in Aristotelian parlance is conceptualized as distinguished from “violent motion” that is clearly caused by an external mover. However, it soon transpires that Fabri’s true goal is to reframe the Aristotelian discourse and adapt it to contemporary theories and results, mainly those of Galileo and his followers. First, while speaking of “natural motion” no reference is made to either upward motion or to levity. According to Fabri, the only natural motion which exists is motion downwards, caused by gravity (*gravitas*) (ibid.: 74).⁴ And indeed, in his *Physica, id est Scientia rerum corporearum* (published in 1669, but reflecting Fabri’s ideas of the 1640s), in a section entitled *De gravi et levi*, Fabri declares that “every body is heavy by absolute gravity”, and that not only does “absolute levity not exist”, but also that – regarding motion of heavy things – “Aristotle was not as accurate as one would perhaps wish, in what was indeed proved by Galileo as a complete falsehood” (Fabri 1669: 254).⁵ In the same text Fabri explicitly mentions Archimedes (as well as Galileo and Torricelli) as a supporter of his view denying an intrinsic property of levity (ibid.: 267).⁶ In fact, he adopts Galileo’s well known ‘dynamic’ interpretation of Archimedean hydrostatics – which involves associating weight with density and considering the causes held by Archimedes responsible for equilibrium as effecting natural motion – and insists accordingly that levity is only a relative property. He explicitly states that

Indeed I admit that Aristotle has written some things about gravity that contradict manifest experiences and arguments. For instance, that of two unequal bodies, of the same material and shape, the bigger descends faster; that of two equal bodies, but of a different material, the heavier descends faster according to the proportion in which it is heavier; that the same heavy object descends through diverse media according to the proportion in which one medium is denser than the other. (Ibid.: 267–268)⁷

4. “Motus localis naturalis est, qui est a gravitate deorsum”.

5. “Propositio XXVIII: Omne corpus est grave gravitate absoluta ... Immo ostendam infra, nullam dari levitatem absolutam; & vero, ut dicam quod res est, in iis quae pertinent ad gravium, non tam accuratus fuit Aristoteles, quam aliquis forte desideraret, in quo sane multae falsitates a Galileo convictus est”.

6. “Huic sententiae authores non desunt. Magnus Archimedes ad instar omnium esse posset; nempe ille primus gravitatis principia ponderavit; huic accedit Galileus, in libro quem de in-
nantibus inscripsit, cui adde successorem Torricellum ...”.

7. “Equidem fateor Aristotelem nonnulla de gravitate scripsisse, quae manifestis repugnant experimentis & rationibus; v.g. ex duobus corporibus inaequalibus, eiusdem materiae, &

In another book, *Metaphysica demonstrativa* (1648), Fabri also criticizes Aristotle for examining only gravity, and failing to consider the acceleration of falling bodies (Fabri 1648:274);⁸ this criticism can be seen as diametrically opposed to his critique of Galileo, who according to the Jesuit (as we have seen above) did well in mathematically exploring acceleration but produced an analysis which is not 'physical'.

In spite of the criticisms against Aristotle, which amount to a rejection of all his major theses on free fall, Fabri's strategy is to avoid a head-to-head confrontation with the Philosopher. Rather, he tries first to argue that in fact the Aristotelian position is reconcilable with the Archimedean one since for Aristotle "heavy and light are the same as dense and rare" (Fabri 1669:267).⁹ But, he continues:

There is nothing wrong in deviating from Aristotle if either a manifest experience or an evident argument compels [us to do so], and indeed he says many things to which are opposed either divine faith or manifest argument and experience. (Ibid.:268)¹⁰

Furthermore, Fabri was not alone among Jesuit mathematicians to adopt the Archimedean framework without rejecting the Aristotelian while inventing strategies for their reconciliation. Thus Paolo Casati in his *Mechanicorum libri* (published in 1684, but actually based on a course given at the Collegio Romano in the early 1650s) dedicated a whole chapter to the problem of levity, at the end of which he concludes that:

Every movement, which is perfected in the same line, includes absolutely similar ubications which are acquired successively, be it ascent or descent – while the order of acquiring is the only difference. Ascent, therefore, differs from descent only in that: which ubication the stone obtains at last, after the other ubications

figurae, maius velocius descendere; ex duobus aequalibus, sed diversa materiae, gravius in ea proportione velocius descendere, in qua est gravius; idem grave per diversa media, in ea proportione descendere, in qua unum medium alio densius est".

8. "Quod vero spectat ad gravia, quae deorsum eunt, consideravit tantum Aristoteles impetum innatum, quo primum moventur, vel gravitant, qui est a generante; nec enim consideravit motum acceleratum".

9. "Venio ad Aristotelem, qui multis in locis nobis aperte favet ... ubi dicit grave et leve idem esse quod densum & rarum". Aristotle's remark is indeed hard to understand within his general scheme which depends on the absoluteness of both gravity (or heaviness) and levity.

10. "Porro discedere ab Aristotele, si quando vel manifestum experimentum, vel evidens ratio cogit nullum vitium est, & vero multa dicit, quibus vel divina fides adversatur vel manifesta ratio, & experientia".

towards the end of the movement. In order to change this order it seems that a dissimilitude of the moving force is not necessary, because actually nothing dissimilar happens. So if someone says that the variety of conditions is enough to explain it, it will not be without reason. (Casati 1684: 11–12)¹¹

Thus Casati undermines the whole debate about “positive levity”, claiming it to be terminological rather than substantial. One can negate positive gravity as well as positive levity. No body, concludes Casati, should be considered as either “heavy” or “light” in an absolute manner: “heavy” is always heavy in relation to something, and within a certain medium. It is the movements of bodies that are called *gravitare* and *levitare* according to their direction; their direction is towards their natural place, which is not arbitrary, but geometrically determined as the center or the periphery of the sphere, depending on their density or rarity. The use of the term *ubicatio* instead of “place” eliminates any qualitative difference between ascent and descent (Feldhay and Even Ezra forthcoming). In fact, relinquishing levity as an ‘absolute’ entity was so popular in Jesuit circles, that the Jesuit Pierre Le Cazre tried (albeit in vain) to prevent the Jesuit authorities from including it among the condemned opinions of the *Ordinatio pro studiis superioribus*, issued in 1651 (Hellyer 2003: 28–32; see below).

As a proponent of the concept of impetus, it is important to Fabri to establish the *inner* cause of natural motion (Fabri 1646: 76),¹² against both Aristotle’s and modern mechanistic explanations, which ascribed motion to *external* causes; he therefore takes pains to attack such ‘external’ arguments. Fabri rejects, for instance, one of the arguments that Galileo, declining to engage in causal inquiry, had defined as “fantasies”: Aristotle’s assertion that the air “extrudes” the body downwards. Fabri simply claims that the surrounding medium resists, and not assists, the motion of a body moving in it (Galilei 1989: 159; Fabri 1646: 77). Another opinion which Fabri criticizes claims that natural motion downwards

11. “Quandoquidem motus, qui in eadem linea perficitur, similes plane includit ubicationes successive acquisitas, sive ascensus sit, sive descensus, ordine tantum in earum adeptione, commutato. Quare cum ascensus a descensu hoc uno differat, quod quam ubicationem lapis demum obtineret post alias prope finem motus, si fuisset centro propior quam mercurius, eam acquirat sub initium motus ante alias, si in mercurii locum aer aut aqua surrogetur centro vicinior quam lapis: ad ordinem hunc permutandum non videtur necessaria virtutis motricis dissimilitudo; nihil quippe producitur dissimile. Sed si quis sufficere dicat conditionum varietatem, nihil absonum forte loquatur: debuit enim una virtus activa in sui effectus productione non uni tantum conditioni alligari, sed pro earum varietate modum quoque operandi mutare posse, modo præstitutos fines, quoad substantiam, non transiliret”. Translated by Rivka Feldhay and Ayelet Even Ezra; our emphasis.

12. “Datur motus localis naturalis, isque ab intrinseco”.

is caused by “a pushing force of some kind, which some assign to the heavens” (Fabri 1646: 78),¹³ that is, Fabri rejects here Descartes' well known explanation of *gravitas* in terms of “subtle matter” (Descartes 1984–1991, vol. 1: 269).

Fabri's effort to exclude external causes relative to free fall and to establish an internal one should be understood against Aristotle's own difficulty to interpret the motion of both light and heavy bodies. As is well known, in the Aristotelian discourse the most universal principle for explaining motion of inanimate objects is “whatever moves is moved by something else”.¹⁴ But in the case of heavy and light bodies the moving agent is not readily apparent. In his explanation, then, Aristotle pointed out the “forms” of light and heavy bodies – their tendency to move to the center of the universe or away from it – and referred to them as the “potentialities” whose actualization results in upward and downward motion. Such tendencies, however, were not the causes but the principles of motion: according to Aristotle, the ‘real’ (i.e., efficient, and not accidental) cause was external and identified with the *generans* – “that which brought the thing into existence as such and made it light and heavy” (*Physics*, 8, 4, 255a24–256a4).

Fabri here chooses again not to directly confront the Aristotelian tradition, but rather to emphasize that the origin of his ‘internal’ theory could actually be found in Aristotle's account. Referring to the “stricter Peripatetics” (*severiores Peripatetici*), the followers of the traditional ‘external’ view, Fabri claims:

Surely, those who attribute the motion of heavy things to the *generans* as a principle cause, do not deny that inside heavy things there exists gravity (*gravitas*), which would be a less principal active principle (*principium activum minus principale*) of motion. (Fabri 1646: 79)¹⁵

Fabri now draws the conclusion important to him: everyone agrees, he claims, that “natural motion exists, which arises from an immediate intrinsic power”

13. “Quarto, motus naturalis non est a virtute quadam pellente, quam caelo quidam affingunt”.

14. “Everything that is in motion must be moved by something”, declares Aristotle (*Physics*, 7, 1, 241b24), and later explains that only animals are “those whose motion while natural is derived from themselves” (*Physics*, 8, 4, 254b27, 255a5).

15. “Sexto, sunt denique multi, iique ex severioribus Peripateticis, qui existimant gravia moveri deorsum a generante, quod expressis verbis traditum est ab Aristotele l. 8. phys. cap. 4. iuxta principium illud universalissimum: Quidquid movetur, ab alio movetur ; sed profecto ii ipsi, qui motum gravium generanti tribuunt, tanquam principi causae, non negant inesse gravibus gravitatem, quae sit principium activum minus principale motus”.

(ibid.).¹⁶ Thus Fabri transformed Aristotle's dictum (pertaining to inanimate objects), *quidquid movetur, ab alio movetur*, to *quidquid movetur, ab aliquo movetur*, as far as local motion is concerned. This transformation, of course, is not an original contribution of Fabri's theory: it is already implied in the fourteenth-century theory of impetus, which identified it as the cause of natural acceleration (Maier 1982: 52); but perhaps ironically, the rejection of the 'external' causality of natural acceleration – and thus the choice not to resort to impact-based explanations (like Descartes and his 'mechanistic' followers, especially Gassendi) – will indirectly assist him in his primary aim to ultimately assimilate Galileo's law of fall.

2.2 The law of natural numbers and the convergence to Galileo's law

We have so far followed Fabri's attempts to reframe the Aristotelian discourse on natural motion by excluding levity and motion upwards, by adopting the Galilean dynamic interpretation of Archimedean hydrostatics to account for falling bodies, and by rejecting external causes to the motion of fall. These bring him to the conclusion that "a heavy body is not moved by a first cause ... not by the air, not by a magnetic force ... not by the heavens pushing them, nor by their *generans* immediately" (Fabri 1646: 79).¹⁷ Having brushed aside every possible extrinsic cause for natural motion, he reaches the inevitable and sought after conclusion, that this cause must be internal (ibid.).

But it is important to emphasize again that although Fabri rejects all the Aristotelian theses on falling bodies he avoids a general confrontation with Aristotle. Rather, he tends to present his theses as corrections to inaccuracies or mistakes of Aristotle or the "stricter Peripatetics". Thus he considers his own presentation as a modification of the Aristotelian discourse, and not as an attempt to advance a new theory. At the same time, the rejection of an external cause keeps Fabri away from the mechanist supporters of Descartes – especially Gassendi – who tried to incorporate Galileo's law of fall into their 'impact model' view but failed (Palmerino 1999: 273, 311–312).

It is time to show how Fabri establishes impetus as the internal cause of natural acceleration and develops his 'natural numbers' law, according to which in each successive (equal) period of time a falling body passes a distance which is one unit bigger than its immediately preceding one, following in fact the simple

16. "In hoc quippe conveniunt omnes tum sectarum Principes, tum recentiores: quidquid sit etiam ex iis ipsis datur motus naturalis, qui est a virtute proxima intrinseca".

17. "Igitur non movetur corpus grave a causa prima ... nec ab aere, nec a virtute magnetica ... nec a caelo pellente, nec a vi sympathica, nec a generante proxime & immediate".

series 1, 2, 3, 4, 5 etc. First Fabri claims that natural motion is “something really distinct from the mobile” (otherwise causal analysis, and the whole concept of impetus, would be superfluous) and that it does not arise from the “entity of the mobile” (*entitas mobilis*) or from “gravity itself” (*ipsa gravitas*), because these are constant causes, while the speed of natural motion is of course inconstant (Fabri 1646: 79–80).

Theorem 5 finally discloses the cause for free fall: “Hence natural motion arises from impetus”. Having rejected every possible external explanation, as well as internal “entity” and *gravitas* only impetus is left, “because there is nothing else intrinsic by which motion could arise, according to the third definition of the first book” (Fabri 1646: 80).¹⁸ Fabri’s claim that he has effectively refuted every (external and internal) remaining reason for natural motion – except for impetus – is of course not very convincing. It is this type of reasoning which would prompt Leibniz to demand from Fabri, his Jesuit correspondent and 38 years his senior: “I would like, however, that you were little more severe in demonstrating than you have been” (Leibniz 2006: 237).

But now a new question arises: how is this impetus created? Impetus has to be occasioned by an ‘intrinsic cause’, explains Fabri, otherwise natural motion itself would be extrinsic – an assumption already refuted earlier; he finally claims that the only possible cause for this impetus is “the substance of the heavy body itself (*substantia corporis gravis*)” (Fabri 1646: 81). Fabri subsequently easily formulates the process by which the impetus (and therefore the velocity, which depends directly on impetus) increases: the substance of the body creates, in each instant, an additional unit of impetus and therefore the velocity increases (*ibid.*: 82). Fabri emphasizes that this primary cause – the body’s substance – cannot be responsible for the duration of each newly created impetus; otherwise it could not be regarded as responsible for the accumulation of impetus, for a cause can have only one effect at each time. Therefore the substance of the body is responsible solely for the accumulating impetus, while the factor which is responsible for the conservation of impetus is God (*ibid.*: 81).¹⁹ Fabri’s differentiation between *impetus innatus*, which he identifies with *gravitas* as the mere inclination of bodies to the center of the earth, and between this accumulated impetus – which he defines as *impetus acquisitus* (*ibid.*: 83) – allows him to perform a rigorous mathematical analysis of free fall, and ultimately prove its convergence to Galileo’s law.

18. “Hinc motus naturalis est ab impetu. Probatur; est ab aliqua causa per Ax. 8. lib. 1; ab aliqua intrinseca per Th. 1; non a substantia corporis gravis per Th. 3; non a gravitate per Th. 4; igitur ab impetu, quia nihil aliud esse potest intrinsecum, a quo sit motus per definitionem 3 lib. 1”.

19. This is of course a Cartesian idea; Fabri is no doubt influenced by Descartes’ assertion that God is responsible for the conservation of the “Quantity of Motion”.

One of Fabri's theorems on this subject is particularly important, because it exemplifies the great difference between Buridan's explanation of free fall – which also involves impetus – and Fabri's theory, against the firm assertion of some historians (especially Stillman Drake and David Lukens that the two are “hardly distinguishable”²⁰). Theorem 13 states:

The impetus produced in the second instant in a free medium is conserved in the third, that produced in the third instant is conserved in the fourth, and so on, because they are not conserved by the initial productive cause ... nor does anything exact its destruction: not a contrary impetus, none of which of course is applied, nor a resistance of the medium, which indeed exists to some amount; but certainly not so much as to be able to entirely impede motion; for I suppose a free medium, therefore also that it does not destroy impetus; since clearly as long as the effect endures, so does the cause; therefore there is nothing which would exact the destruction of that impetus. (Ibid.: 82)²¹

Fabri's ability to neglect all impediments to motion (especially air resistance) is significant. It is a far cry from the approach of Buridan, the famous protagonist of the concept of impetus, who forbids motion in a void, cannot visualize a resistance-less ‘ideal’ situation of linear terrestrial motion, and is therefore unable to separate, in his physical account, motion from resistance – a critical step in formulating a rigorous mathematical analysis (Elazar 2008:28–33; Clagett 1959:539, 560–561).

Fabri's conception, which explicitly assumes an ‘ideal’ straight motion that would occur under the assumption of void, is directly conveyed in his remark later in *De motu naturali deorsum* (the second book of *Tractatus physicus*), having completed the analysis of free fall: “If the heavy body were to descend in vacuum, the abovementioned proportions would be preserved very accurately, because there is no impediment; but indeed if any impediment intervenes, no doubt they

20. See Lukens (1979: 161, n. 48). Drake sees Fabri's theory as merely a natural (not to say trivial) “logical development” of Albert of Saxony's theory, which was itself “the most natural, and perhaps the only consistent, mathematization of Buridan's impetus theory of fall” (Drake 1974: 50, 56).

21. “Impetus productus secundo instanti in medio libero conservatur tertio, & productus tertio conservatur quarto, atque ita deinceps; quia scilicet nec conservantur a causa primo productiva ... nec aliquid exigit destructionem; non contrarius impetus, quia nullus est applicatus, ut constat; non resistentia medii, quae quidem alicuius momenti est; sed non tanti, ut impedire possit motum omnino, ut constat; nam suppono liberum medium, igitur nec destruere impetum; cum tamdiu duret causa quamdiu durat effectus, ut patet; igitur nihil est quod exigit impetus huius destructionem”.

are not preserved accurately" (Fabri 1646: 115).²² Such a conception, utterly alien to Buridan's view, reveals the deep influence of the New Science of the seventeenth century, and is part of what could be defined as Fabri's 'inertial framework': he in effect embraces Descartes' two first laws of motion, and incorporates into his impetus-based physics the principle of linear conservation of motion (often hastily defined as 'inertia') in the absence of resistance (Elazar 2008: 21).²³

Fabri is now well on his way to prove his 'natural numbers' law. He first establishes that free fall involves uniform acceleration, claiming that "in equal times equal impetus is produced", because the same necessary cause acts under constant circumstances, then that "in the same proportion that impetus increases, the motion is accelerated, because in the same proportion that the cause increases so does the effect" (Fabri 1646: 85).²⁴ He concludes that "impetus increases according to an arithmetical progression, for single instants add equal impetus", while the speed "increases in the same way, since in single instants equal additions (*momenta*) of velocity are acquired" (ibid.: 88).²⁵ After relying on the simple rule according to which if two uniform motions occur in equal times, the spaces traversed are in relation to each other as the velocities, Fabri easily deduces:

Hence the spaces increase in single equal instants according to an arithmetical progression, because space increases as the speed ... and speed as the impetus ... and this according to an arithmetical progression. Hence the spaces acquired in single instants are as the series of numbers which compose the simple progression, 1, 2, 3, 4, 5, 6, etc. (Ibid.)²⁶

22. "Theorema 71: In vacuo si corpus grave descenderet, praedictae proportiones accuratissime servarentur; quia scilicet nullum esse impedimentum; at vero si aliquod intercedit impedimentum; haud dubie non servantur accurate".

23. It is also worth mentioning that Buridan, unlike Fabri, permitted not only linear impetus but also a circular one (Clagett 1959: 534).

24. "Theorema 17: Aequalibus temporibus aequalis impetus producit, si sit eadem applicatio, idemque impedimentum probatur, quia causa huius impetus est necessaria; sed eadem causa necessaria aequalibus temporibus aequalem impetum producit Theorema 18: Qua proportionem crescit impetus acceleratur motus; quia quae proportionem crescit causa, etiam crescit effectus ..." Cited from Lukens (1979: 160).

25. "Theorema 37: Hinc crescit impetus iuxta progressionem arithmetica; cum singula instantia aequalem impetum addant ... Theorema 38: Eodem modo crescit velocitas, quia singulis instantibus aequalia acquiruntur velocitatis momenta".

26. "Hinc spatia crescunt singulis instantibus aequalibus secundum progressionem arithmetica; quia crescit ut velocitas per Th. 40. haec ut impetus per Th. 38. hic demum iuxta progressionem arithmetica per Th. 37 ... hinc spatia acquisita singulis instantibus sunt ut series numerorum, qui componunt progressionem simplicem, scilicet 1.2.3.4.5.6. &c". Cited from Lukens (1979: 175).

Now Fabri takes pains to show that assuming very small instants, his law converges to the ‘odd numbers rule’, that is Galileo’s law of falling bodies. We have already met Fabri’s claim that in equal times of descent a falling body acquires equal additions of velocity, thus incorporating Galileo’s simple proportionality between velocity and time. Analyzing this proportion from his discrete point of view, Fabri concludes that new impetus is acquired “in single equal instants”, and thus each instant produces an extra unit of velocity and therefore an extra unit of space. Now Fabri develops a discrete ‘mirror image’ of Galileo’s ‘continuous’ discussion; for example, he shows that Galileo’s well known ‘mean speed rule’ (Galilei 1989: 165),²⁷ and ‘double-distance rule’ (ibid.: 168)²⁸ are equally valid in his discrete analysis. Furthermore, Fabri announces that he accepts Galileo’s experimental results wholesale, and shows that if the instants in which his ‘natural numbers law’ is valid are considered to be small enough, then his law is indistinguishable from Galileo’s ‘odd numbers rule’ (see Figure 1) (Fabri 1646: 102).

Fabri’s discrete counterpart to Galileo’s ‘continuous’ analysis appears immediately after Theorem 61 of *De motu naturali deorsum*, which states the core of Fabri’s objection to Galileo’s continuous approach to velocity: “naturally accelerated motion”, Fabri explains, “is not propagated through every degree of slowness”. Accordingly, Corollary 6 rejects Galileo’s odd numbers rule (Fabri 1646: 96–97).²⁹ Yet it was just explained that Fabri’s analysis shows the convergence of Fabri’s law to Galileo’s; it is worthwhile then to observe the corollaries that conclude this long analysis. The first two effectively undermine (or at least mitigate) Fabri’s decisive objection to Galileo’s assumptions of infinite ‘mathematical’ instants and “infinite degrees of slowness”:

27. “The time in which a certain space is traversed by a moveable in uniformly accelerated movement from rest is equal to the time in which the same space would be traversed by the same moveable carried in uniform motion whose degree of speed is one-half the maximum and final degree of speed of the previous, uniformly accelerated, motion”.

28. If, following naturally accelerated motion (lasting for time AC), a body continues “to be moved with the same degree of speed BC, without accelerating further, then in the ensuing time CI it would pass a space double that which it passed in the equal time AC with degree of uniform speed EC, one-half the degree BC”.

29. “Theorema 61: Motus naturaliter acceleratus non propagatur per omnes tarditatis gradus ... Corollarium 6: Sexto reiiciet illorum sententiam, qui volunt accelerationem motus naturalis ita fieri, ut spatia temporibus aequalibus acquisita sequantur seriem numerorum imparium 1.3.5.7.9.11.13. &c. & spatia sint ut quadrata temporum”.

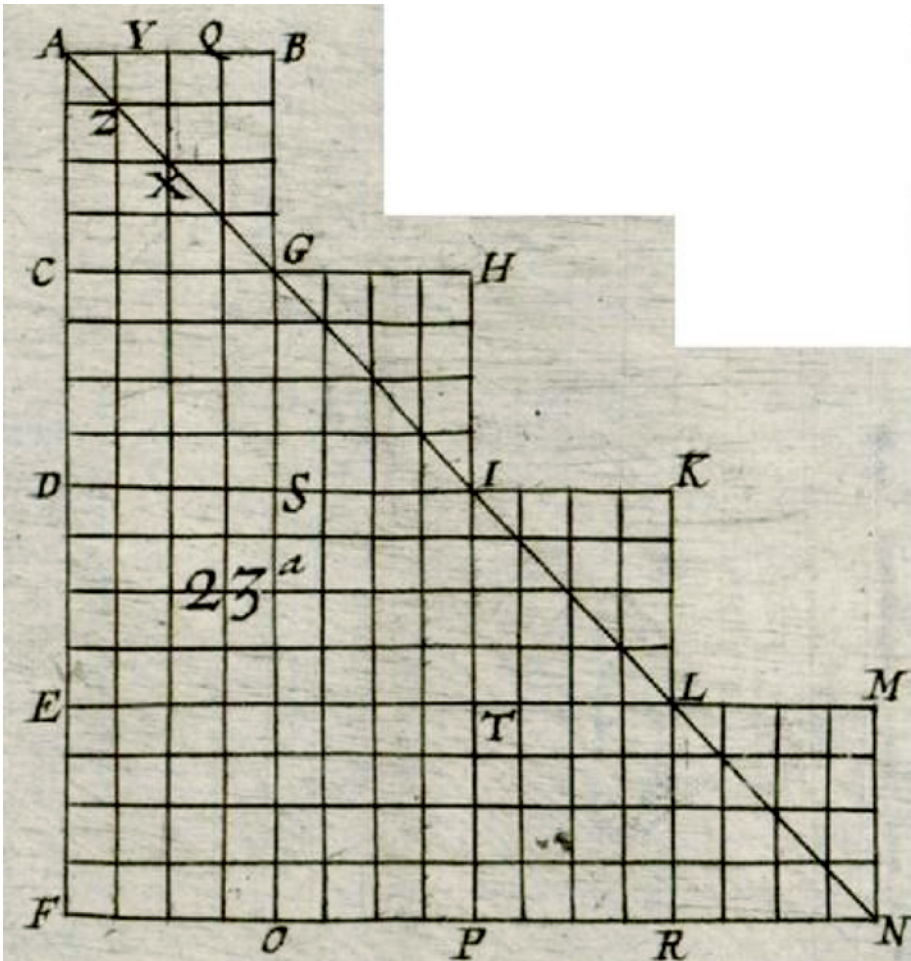


Figure 1. Taken from Fabri's *Tractatus physicus*: http://nausikaa2.mpiwg-berlin.mpg.de/cgi-bin/toc/toc.x.cgi?dir=fabri_tract_026_la_1646&step=thumb): Fabri supplies, in his proof, an arithmetic equivalent to a simple geometrical fact. The horizontal axis is time, the vertical – velocity. Let us assume that RN is a unit of time – the spaces traversed (i.e. the square RLMN and the rectangles PIKR, OGHP, FABO) represent his 'natural numbers' law, while the triangle LRN and the consecutive trapezoidal areas (PILR etc.) of course behave like 1, 3, 5, 7 – Galileo's law.

It is clear – actually trivial – that if we choose smaller units of time, e.g. the little squares, we shall get closer to the straight line AGILN, and ultimately (for infinitesimal instants) the discrete picture converges to a continuous one.

Corollary 1: Although there are not infinite parts of time, nevertheless in practice they behave as if they were infinite; since although they are finite, they cannot be counted.

Corollary 2: Although there are not infinite degrees of slowness but a finite number; in practice they behave as if they were infinite; because the first (and the smallest) cannot be distinguished from all the others. (Fabri 1646: 108)³⁰

The third corollary summarizes Fabri's effort to assimilate Galileo's law of fall:

Corollary 3: Although Galileo's hypothesis is false according to the hypothesis of finite instants (for a new increase of speed occurs in single instants), nevertheless physically speaking it behaves as if it were true; because although it can be proved only in sensible parts of time, surely, since any sensible part contains almost innumerable instants, in which the propagation occurs, there cannot exist a sensible difference between the two [hypotheses]. Therefore the denticulated line [the line with the 'steps'] behaves physically, i.e. sensibly, as if it were straight. (Ibid.)³¹

Finally, Fabri concludes that

in the common opinion in which it is said that time consists of actually infinite parts, Galileo's progression can stand. Therefore, here is the key to the difficulty: the simple progression [i.e. Fabri's natural numbers series] is based on a physical principle, not on experiment; the progression of odd numbers is based on experiment, not a principle. We combine the two, by principle and by experiment; for the first is transformed to the second if sensible parts of time are assumed, and the second to the first – if ultimate instants are assumed. (Ibid.)³²

30. "Corollarium 1: Etiam si non sint partes infinitae temporis; in ordine tamen ad praxim eodem modo se habent, ac si essent infinitae; quia licet finitae sint, numerari tamen non possunt. Corollarium 2: Etiam si non sint infiniti tarditatis gradus, ut constat ex dictis, sed finiti; in ordine tamen ad praxim eodem modo se habent, ac si essent infiniti; quia non potest distingui primus, & minimus ab omnibus aliis".

31. "Corollarium 3: Licet hypothesis Galilei sit falsa in hypothesi instantium finitorum; nam singulis instantibus nova fit velocitatis accessio; physice tamen loquendo eodem modo se habet, ac si esset vera; quia cum non possit probari, nisi in partibus temporis sensibilibus; certe, cum quaelibet pars sensibilis innumera fere instantia contineat, in quibus fit progressio; differentia utriusque sensibilis esse non potest; igitur linea denticulata eodem modo se habet physice, hoc est sensibiliter, ac si esset recta".

32. "in communi illa sententia, in qua dicitur tempus constare ex partibus actu infinitis, progressio Galilei tantum locum habere petest; igitur haec esto clavis huius difficultatis; progressio simplex principium physicum habet, non experimentum; progressio numerorum imparium experimentum non principium; utramque cum principio & experimento componimus; prima enim si assumantur partes temporis sensibiles transit in secundam, secunda in primam, si ultima assumantur instantia".

Thus Fabri managed to offer what seemed to him a physically substantiated law – justifiable by the use of the ‘legitimate’ concept of impetus – which was superior to Galileo’s ‘purely mathematical’ odd numbers law and yet converged to it if small enough time units are assumed. Corollaries 4–6 urge the readers (Fabri’s College students) to effectively abandon the discrete approach, and use Galileo’s assertion that the distance is proportional to the square of the time ($s \propto t^2$) (ibid.: 108–109). Corollary 6 explains that “knowing the time, the covered space can be known; since the spaces are as the squares of the times; or knowing the space, the time can be known; since the times are as the square roots of the spaces” (Fabri 1646: 109).³³

Furthermore, concluding the issue of free fall within vacuum, Fabri restates (in Theorem 70) that “if equal sensible spaces are assumed, the times are almost as the square roots of the spaces; for since the spaces are sensibly as the squares of the times, certainly the times are as the roots of those squares, i.e. spaces”. He provides some examples to demonstrate the applicability of Theorem 70, and winds the discussion up by stating that this proportion should be employed when dealing with sensible spaces, and that “from now on, whenever it is useful, we shall employ it” (Fabri 1646: 115).³⁴

Fabri indeed keeps his promise to continue to “employ” Galileo’s law. In the fourth (and last) appendix of the *Tractatus*, which discusses the “principal of the duplicate ratio”, Fabri marvels at the “many natural effects” to which the “duplicate ratio” – the quadratic ratio – corresponds. The first example he gives is “straight naturally accelerated motion”, in which “spaces are traversed in keeping with a duplicate ratio, i.e. according to the squares of the times” (ibid.: 443).³⁵ Thus, having proved that his law converges to Galileo’s law (in ‘sensible’ times), Fabri has in effect deserted his initial ‘discrete’ assumption that the traversed spaces behave according to the simple natural numbers series.

33. “Corollarium 6: Similiter cognito tempore cognosci potest spatium decursum; quia spatia sunt ut quadrata temporum; vel cognito spatio cognosci potest tempus; quia tempora sunt, ut radices spatiorum”.

34. “Si assumantur spatia sensibilia aequalia, tempora sunt fere in ratione subduplicata spatiorum; cum enim spatia sint ut quadrata temporum sensibilibus; certe tempora sunt, ut radices istorum quadratorum, scilicet spatiorum ... igitur in praxi quae tantum fit in spatiis sensibilibus haec progressio adhibenda est, illamque deinceps, si quando opus est, adhibebimus”.

35. “De principio physico rationis duplicatae physicae: Vix credi potest quam multis effectibus naturalibus haec duplicata ratio affigatur, aliquos cursim indicabo ut verum germanumque illius principium statuatur. 1. In motu recto naturaliter accelerato, decursa spatia sunt in ratione duplicata temporum, id est ut temporum quadrata ...”.

3. Fabri's argument in context

The structure of Fabri's argument has served as a key for interpreting its meaning. The first step in this argument comprises an attempt to modify or even reform the conceptual toolbox of the Aristotelian discourse while keeping some of its formal rules intact. Thus, for Fabri natural motion is downward motion. There is no upward natural motion and no absolute levity in his world. Fabri adopted the Archimedean definition of gravity and levity in terms of 'specific' (or relative) weight. He also argued that this adoption was not contrary to Aristotle's view which in effect acknowledged only weights of bodies in material media. However, by completely identifying levity with rarity and gravity with density, along with his adoption of the 'inertial framework', he was actually closer to the strongest opponents of the Aristotelian theory of matter – the atomists – than to Aristotle himself. Likewise, for Fabri, the most universal Aristotelian law of motion – "whatever moves is moved by something else" – did not apply for local motion. Fabri insisted that the cause of natural motion was internal rather than external and in this he challenged not only the traditional Aristotelian discourse on motion but also contemporary 'mechanical' philosophers who analyzed every motion in terms of impact stamped on the body from the outside. As we have shown, even though Fabri rejected all major Aristotelian theses on the motion of fall, he still reflected on this discursive move as a correction to Aristotle rather than an attempt to undermine the authority of the Philosopher. With this strategy he hoped to maintain, at least formally, the rule that imposed on all Jesuit educators to stay as close to Aristotle as possible (especially in philosophical teachings).³⁶

The second step in Fabri's argument should be understood as an attempt to create for the Jesuits a peculiar position in the cultural field by which they could appear as critical both of some parts of their Peripatetic-Scholastic medieval predecessors and of Galileo and his followers. Thus, Fabri flatly rejected the medium as the cause of motion proposed by the "stricter Peripatetics", and although adopting Buridan's concept of impetus, the Jesuit's fresh 'inertial framework' allowed him to turn Buridan's ambiguous and qualitative account of free fall into a rigorous mathematical analysis which could ultimately be shown to comply with Galileo's indisputable experimental results. But Fabri also disagreed with Galileo's refusal to deal with the cause of natural acceleration as irrelevant for understanding its mathematical properties. He also rejected Galileo's analysis of the mathematical continuum as composed of actual infinite indivisibles, and hence criticized the Italian's odd numbers law of fall. The second step thus

36. Thus, the *Ratio studiorum* of 1599 explicitly instituted that "He [the professor of philosophy] shall not depart from Aristotle in matters of importance" (Farrell 1970: 40).

presents Fabri's theory of the cause of fall in terms of *impetus innatus* and *impetus acquisitus*, in terms of his discrete analysis of time, velocity and space, and his law of natural numbers to describe the uniform additions of velocity during free fall. Thus, after the second stage of the argument is over, Fabri's theory of impetus and his mathematical formulation of the law of fall seem antithetical to both Peripatetic and Galilean discourse.

However, the argument now reaches its third stage, where it comes to its dialectical *Aufhebung*, to borrow a well-known Hegelian notion. Having accepted the dynamical interpretation of hydrostatics as a model for explaining free fall Fabri adopts Galileo's strategy of decreasing the effect of the resistance and concentrating on the limit case of a void. Moreover, he also accepts Galileo's experimental results concerning the behavior of acceleration in free fall. He sets out to show the convergence between his own law and Galileo's law of fall, and even sees fit to praise the quadratic proportion between distances and times. Thus Fabri not only succeeded in assimilating Galileo's positive results but he also showed his philosophical sensitivity to the role of experiment vis-à-vis the role of theory in the construction of scientific discourse. It should be emphasized that before Newtonian and Leibnizian calculus acquired some theoretical basis, Galileo's analysis of the continuum was criticized not only by the Jesuits, but by many prominent figures, among them Descartes, Gassendi, Roberval, and Mersenne (Palmerino 1999: 310–311, 319–322). Thus it is not surprising that Fabri saw his theory as a solution on a higher level of the Jesuits' controversy with Galileo and the Galileans concerning the cause of fall and the analysis of the continuum.

It should further be noted that while no Aristotelian or Thomistic fundamentalism should be projected on Jesuit *savants* (Feldhay 1995: Chapters 7, 8; Feingold 2003: vii–xi), the Aristotelian discourse to which Jesuit mathematicians and philosophers were committed was not just a body of knowledge. Rather, in the Jesuit environment such discourse had assumed the role of a pedagogical and disciplining core around which the freedom of philosophizing could be balanced against the need to impose some kind of uniformity. Thus, the Jesuit *Constitutions* stated explicitly: "In logic, natural and moral philosophy, and metaphysics, the doctrine of Aristotle should be followed, as also the other liberal arts" (Loyola 1970: 219), a statement that was repeated in the *Ratio studiorum* of 1599 (see note 36).

A reconstruction of the debate that took place within the Society in the sixteenth and seventeenth centuries shows that in spite of various mechanisms of control, devised by the educators of the Society, absolute adherence to Aristotle was never demanded nor achieved, and the majority abided by the rule that "common opinion" or the consensus had to be accepted by individual members. However, between the years 1649–1651 particular tension developed around the demand of some educators and the inner censors of the Society to publish a list

of opinions which are not to be taught, and such a list was indeed issued in 1651 by the General Francesco Piccolomini, within the *Ordinatio pro studiis superioribus*; one of the forbidden opinions was the statement that “gravity and levity do not differ by species, but only according to different degrees” (Pachtler 1894, vol. 3: 93).³⁷

Like other Jesuit mathematicians of his milieu – for example the French philosopher Pierre Le Cazre, who wrote his objections to some items on this list (Hellyer 2003: 38–43) – Fabri considered the modification of meaning of *gravitas* and *levitas* along Archimedean lines as commonplace among Jesuit mathematicians and philosophers. Hence his strategy was to show that in most cases Aristotle himself spoke about heaviness and lightness in a medium and therefore for him, too, gravity and levity were mostly meant in a relative way, forgetting Aristotle’s bitter dispute with the atomists in Book IV of *De caelo*.

The *Ordinatio* also condemned Galileo’s reduction of the continuum to indivisibles, but it also explicitly forbade the view which saw the continuum as composed of extended ‘physical’ indivisibles (unlike Galileo’s non-extended ‘mathematical’ indivisibles)³⁸ – a theory which is not only fully endorsed by Fabri but is deeply rooted in his overall conception of space, time, and motion.³⁹ Therefore there is no reason to suspect that Fabri’s strategy concerning his controversy with Galileo was influenced by these Jesuit authoritative assertions, which are connected to an important and highly sensitive and complex religious matter – the Eucharist.⁴⁰

We believe that our presentation and analysis of Fabri’s perspective on his controversy with Galileo and the Galileans throws a new light on one of the most fundamental controversies during the scientific revolution. Controversy, after all, is a historical construct constituted by the historical players in a specific cultural field. The Galileans tended to construe this controversy as an incommensurable dispute between ancients and moderns. However, the Jesuits’ perception of the

37. “Gravitas et levitas non differunt specie, sed tantum secundum magis et minus”.

38. The *Ordinatio* condemned both the following opinions: “Continuum successivum et intensio qualitatum solis indivisibilis constant” ... “Dantur puncta inflata ex quibus continuum componatur” (Pachtler 1894, vol. III: 92). “Inflated points” refer to extended indivisibles.

39. On Fabri’s belief in ‘physical’ (i.e. extended) indivisibles, while specifically rejecting ‘mathematical’ (i.e. Galilean-style) indivisibles, see, e.g., Corollaries 2 and 4 of proposition 12 in Fabri (1648: 371).

40. ‘Extended’ indivisibles were in fact considered by the Jesuit authorities worse than ‘mathematical’ indivisibles, because they implied that Christ existed in the host a finite number of times (Palmerino 2003: 187).

situation was different. They saw a field consisting of a variety of positions, each characterized by a partial point of view: “stricter Peripatetics” who refused to recognize contemporary experimental results that challenged traditional concepts and ways of thought; ‘mechanical’ philosophers who insisted on an external cause of all motions, including free fall; and Galileans like Torricelli and Michelangelo Ricci who refused to develop a causal explanation of natural acceleration. The Jesuits saw themselves as capable of providing an overall perspective by negotiating and mediating among all these views and synthesizing them within an Aristotelian-Archimedean framework.

With the advent of Newtonian physics the situation changed dramatically, however, and Galileo's law of fall was easily incorporated into a framework that was in fact alien to his own original intellectual environment, which remained Aristotelian-Archimedean to the end. As is almost always the case, history's verdict was on the side of the winners, the Jesuit point of view was discarded and forgotten, while the winners' point of view was reproduced in the work of modern historians. Thus historians like Drake, Lukens, and Galluzzi preferred to neglect the allegedly ‘unimportant’ – from a modern point of view – differences between Peripatetics, conservative Jesuits, and innovative Jesuits on the one hand, and between Galileans and ‘mechanists’ on the other. Instead, they created the dichotomy between the first group – ancients – and the second group – moderns – and constructed an inevitable conflict instead of a controversy.

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Galileo, the Jesuits, and the controversy over the comets

What was *The Assayer* really about?*

Ofer Gal and Raz Chen-Morris

1. Introduction

During the fall of 1618 three comets appeared in the European sky in short succession. The third and most impressive of them, in late November, was carefully observed by astronomers all over Europe. A few months later the Jesuit mathematician Horatio Grassi published, anonymously, a tract titled *De Tribus Cometis Anni 1618 Disputatio Astronomica* (Drake and O'Malley 1960). Limiting himself to the traditional role of astronomy, namely "the single role of the mathematician" Grassi promised "not to exceed the boundaries of our knowledge" and declared that "if I explain the position, motion, and magnitude of those fires, I shall be satisfied that I have fulfilled my purpose" (Grassi "Disputation"; Drake and O'Malley 1960: 6–7). The "position and motion" he assigned to "those fires", however, were far from traditional, that is, if two generations after Tycho and three after Copernicus, one should still take 'tradition' to refer to Ptolemy and Aristotle: "our comet", Grassi declares, "was not sublunar but clearly celestial" (Grassi "Disputation"; Drake and O'Malley 1960: 14).

Tycho Brahe had already arrived at this conclusion concerning the comet of 1577, but the idea that comets were superlunary did not lose its revolutionary cosmological significance: the sharp dichotomy between the realms under and above the moon, which the heavenly position of comets undermined, was not an arcane scholarly conviction; it was well entrenched in religion and commonsense.¹ The

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1. Concerning the significance of the superlunary position of comets see van Nouhuys 1998.



Figure 1. One of the many popular accounts of the comets of 1618: Arthusius' *Cometa Orientalis*.

'disputation' in Grassi's title suggests that he expected contentious reactions, and he indeed found himself under fierce assault, but it did not come from the guardians of tradition. Instead, it came from the person Grassi made a point to hail with the very first lines of the treatise by writing "only comets have remained aloof from these lynx eyes" (Grassi "Disputation"; Drake and O'Malley 1960: 6). It was the eyes of the Lincean Galileo Galilei that Grassi was referring to, and it was Galileo, of all people, who had risen to defend traditional cosmology from the new observations.

That it *was* Galileo did not become immediately clear, because Grassi's anonymous *Disputatio* set off a masquerade. Galileo's initial answer, the *Discourse on the Comets*, was dictated, in Tuscan, through his disciple and fellow academician Mario Guiducci.² Grassi, however, was not fooled. Replying with the *Astronomical and Philosophical Balance*, apparently composed in collaboration with other members of the *Collegio Romano*, he mocked Galileo's games, only to join them by signing with the anagram Lothario Sarsi of Siguenza:

Since Galileo ordered the matter to be discussed through intermediaries and interpreters, he has thereby presented the secrets of his mind not through himself but through Mario the consul of the (Florentine) Academy. ... Thus I, too, am now permitted to present those things which I have heard from my master Horatio Grassi about the recent discoveries of that same Galileo not to the consul but to the students of mathematics ... but ... I have determined, not without justice, that my dispute about those matters is with the dictator rather than with the consul.

(Grassi "Balance"; Drake and O'Malley 1960: 70)

For the celebrated climax of the debate Galileo (who never observed the comets in person) unmasked and answered 'Sarsi's' Latin *Balance (Libra)* with his own – again Tuscan – *Assayer (Saggiatore)*, simultaneously denying that he was behind Guiducci and complaining about Sarsi's lack of manners in exposing him (Galileo "Assayer"; Drake and O'Malley 1960: 169).

2. The arguments

The claims Galileo vehemently contests are carefully measured factual conclusions; Grassi tries hard not to "exceed the boundaries of our knowledge" and remain within the realm of empirical modesty. He was compelled to station the comet in the celestial realm, he explains, by his parallax calculations – the comparison of the angles of sight from different positions:

If the comet was observed from different places and compared with the stars of the firmament, and if it preserved the same distance from them, it must be regarded as either in the firmament or certainly not far removed from it. But if it underwent parallax, it must be placed below the firmament in proportion to the amount of the difference of aspect. (Grassi "Disputation"; Drake and O'Malley 1960: 11)

2. Favaro asserts that "the pages of the first part ... have corrections and additions in Galileo's handwriting. A second part ... is entirely in Galileo's writing. The third part in Guiducci's hand ... but there are correction by [Galileo]" and concludes that "the entire discourse may be said to be essentially [Galileo's] work" (Drake and O'Malley 1960: xvi–xvii).

These calculations gained further support through the intricate network of Jesuit scholars and institutions deployed all over Europe. When observers in “Milan and in Parma ... from Innsbruck in Germany and from France and Belgium” reported the same position for the comet, and when finally two particularly accurate observations from Rome and Cologne completely coalesced in time and place, Grassi felt assured. Well aware of the far-reaching implications of placing the comet in the heavens, however, he buttresses the argument from (the lack of) parallax with three further arguments, all of them anti-Aristotelian in details but Aristotelian in their general outlook and presumptions (cf. Grant 1978). The first is directed against the Aristotelian claim that comets are fiery exhalations, a claim which Galileo would make extensive use of: comets are simply much too large, Grassi calculates, to be fuelled by exhalations from the earth. For Grassi this argument also implies that the light of the comet is not its own “but that of the sun; and the solar rays, either refracted or reflected, proceeded farther on and formed the tail”; “as [Johannes] Kepler would have it”, he adds, displaying his acquaintance with the state of the optical art (Grassi “Disputation”; Drake and O’Malley 1960: 15). Obviously, though he places the comet “in the firmament or certainly not far removed from it”, Grassi assumes that it is more akin to the planets than to the fixed stars. The second argument is that whereas “fiery irruptions have no regular and definite motion ... The motion of our comet was always constant and not unlike the motion of the planets”. The comet’s motion “always follow[s] a middle and splendid pathway”, allowing the astronomer to recognize it as part of the heavenly realm (Grassi “Disputation”; Drake and O’Malley 1960: 17).

It is Grassi’s final argument which arouses Galileo’s ire, and it is this wrath that points to the real issue of the debate. On the face of it, however, Grassi’s argument appears fairly innocuous:

It has been discovered by long experience and proved by optical reason that all things observed with this instrument [the telescope] seem larger than they appear to the naked eye; yet according to the law that the enlargement appears less and less the farther away [the observed objects] are removed from the eye, it results that fixed stars, the most remote of all from us, receive no perceptible magnification from the telescope. Therefore, since the comet appeared to be enlarged very little it ... is more remote from us than the moon, since when [the moon] has been observed through the telescope it appears much larger.
(Grassi “Disputation”; Drake and O’Malley 1960: 17)

Because Galileo retains the masquerade of names and titles throughout his argumentation, it takes some prodding to reveal that this is the argument he aims to refute. Even in the *Discourse*, dictated to Guiducci and before being entrapped

into the Jesuit disputation mode, Galileo adopts positions which have little merit other than as refutations of Grassi's arguments. He ignores altogether Grassi's quasi-quantitative claim about the size of the comet as well as the reference to Kepler. This takes him out of the dangerous zone of planetary theories and the already-contentious issue of Copernicanism it entails, and it makes an easy prey of Grassi's other auxiliary point, that "the motion of our comet was always constant and not unlike the motion of the planets". This claim demanded some strenuous extrapolation of the comet's trajectory, and after all, even the constancy of planetary motion is also a product of much theorizing:

as to the clarity, the observations and statements of these very people have shown the motion not to be regular because of being continually decelerated in such a way that the comet of 1577 was 20 times as fast at the beginning as at the end, and the recent one was about twice as fast.

(Guiducci "Discourse"; Drake and O'Malley 1960: 50)

Grassi's primary argument from astronomical observations – the lack of parallax – receives a different treatment altogether. Galileo does not attempt to undermine the accuracy of the observations or their interpretation. Instead, he assaults the physical assumptions that allow them:

A comet is not one of the wandering stars which become visible in a manner similar to that of some planet.

(Guiducci "Discourse"; Drake and O'Malley 1960: 27)

"Parallax operates reliably", Galileo explains, "in real and permanent things whose essence is not affected by anyone's vision; these do not change place when the eye is moved. But parallax does not function in mere appearances". Comets, on the other hand, are among those

reflections of light, images, and wandering simulacra which are so dependent for their existence upon the vision of the observer that not only do they change position when he does, but... would vanish entirely if his vision were taken away.

(Guiducci "Discourse"; Drake and O'Malley 1960: 36–37)

The suggestion that comets are mere appearances also takes care of Grassi's point about the needed fuel, but Galileo never really commits to the physical makeup of comets. The most he is willing to claim is that "there is nothing among real visible objects which so much resembles a comet as do some of these optical images" (Guiducci "Discourse"; Drake and O'Malley 1960: 39), but this suspicion is enough for him to declare the parallax calculation "nil" (Guiducci "Discourse"; Drake and O'Malley 1960: 40):

if parallax has no cogency in determining the distances of all these refractions or reflections, images, appearances, and illusions because they change place as the observer moves (and change not only their place but their identities), I shall not believe that parallax has really any place in comets until it is first proved that comets are not reflections of light, but are unique, fixed, real, and permanent.
(Guiducci "Discourse"; Drake and O'Malley 1960: 39)

As René Descartes will stress a couple of decades later, some appearances, like the colors of the rainbow, are just that – appearances in essence: "every eye sees a different rainbow, a different halo, or a particular set of mock suns; those which are seen from different places are not derived from the same rays ... but from diverse ones" (Guiducci "Discourse"; Drake and O'Malley 1960: 39). Comets may belong to the same category. Galileo is undoubtedly aware that his claim that comets are sublunary, "originat[ing] in the elemental sphere", will make his reader assume that he is reverting to Aristotle's theory, according to which they are created when condensed hot and dry exhalations are set on fire by the motions of the sky. But Galileo is adamant that this is impossible: the sky has a smooth lower surface so it cannot move the air underneath. Moreover, Galileo insists that motion *per se* cannot cause heat. On the contrary: experience shows that it cools down warm things. Only friction and compression of solid bodies cause heat whereas

I do not believe that mere agitation made in water or in air or in any other tenuous and yielding body could excite heat and fire ... moreover ... friction does not produce heat in all sorts of solid bodies, but only in those of which one or both are consumed or pulverized ... when they are struck together.
(Guiducci "Discourse"; Drake and O'Malley 1960: 31–32)

With *The Assayer* the controversy comes to its virtual end. Grassi still attempts to reply (as Lothario), but apart from complete dismay at Galileo's ingratitude has little new to offer, and Galileo never bothers to respond. The surprise by which Grassi and the Jesuits are taken by Galileo's positions and arguments is very telling, especially considering the common interpretation of *The Assayer*.

3. *The Assayer*

The Assayer occupies a place of honour in the grand epos of modern science, best captured in the words of Galileo's revered translator and editor, Stillman Drake: "the philosophy of Galileo as set forth in *The Assayer* is best characterized by saying that he presented an open system of scientific inquiry in opposition to the closed system of the schools" (Drake and O'Malley 1960: xxiii). The general acclamations of the "admirable clarity and conciseness" (Cassirer 1942: 316) of *The*

Assayer, common in traditional historiography of the ‘Scientific Revolution’, have since then been replaced by more balanced readings, but not much has changed in understanding Galileo’s claims and motivations in the treatise. One line of interpretation finds in *The Assayer* a demonstration of Galileo’s mathematical Platonism (cf. Koyré 1943; De Caro 1993). The other reads it as part of Galileo’s struggle to free Copernicanism from the dictates of scriptural interpretation (cf. Biagioli 206:220ff; Palmerino 2006). Both interpretations concentrate on its two famous paragraphs:

Philosophy is written in this grand book – I mean the universe ... in the language of mathematics. (Galileo “Assayer”; Drake and O’Malley 1960: 183–184)

and

I do not believe that for exciting in us tastes, odours and sounds there are required in external bodies anything but sizes, shapes, numbers, and slow or fast movements; and I think that if ears, tongues, and noses were taken away, shapes and numbers and motions would remain, but not odours or tastes or sounds. These, I believe, are nothing but names, apart from the living animal just as tickling and titillation are nothing but names when armpits and the skin around the nose are upset. (Galileo “Assayer”; Drake and O’Malley 1960: 311)

Neither interpretation pays much attention to the controversy over the comets, which comprises the context to *The Assayer* or to the positions Galileo and his adversaries take in the debate. In his *Galileo’s Intellectual Revolution* William Shea presents a rare exception, but even he takes Galileo’s surprising positions as sophisticated rhetorical moves in the service of his Copernicanism (Shea 1972: 86–87). In general, *The Assayer* is still customarily referred to as it was by Ernest Cassirer (1942: 316): “the polemic against the Aristotelian and scholastic physics”.

By 1623 Galileo was definitely of the Copernican persuasion³ and had already formulated a public defence for the supposed discrepancies between Copernicanism and the Scripture. He had undoubtedly challenged “Aristotelian and scholastic physics”, and his thorough trust in the power of mathematics may even merit calling him a Platonist. Yet very little of this is relevant to *The Assayer*. First, because the only mention of scriptural interpretation by either side of the controversy is a casual reference by Grassi to the advantage of Tycho’s over Copernicus’ system in that respect. Nowhere in *The Assayer* does Galileo present or refer to anything like his famous arguments about scriptural interpretation from the “Letter to the Grand Duchess” (Finocchiaro 1989: 87–99). Secondly, because Galileo explicitly

3. This was not always the case: in his *De Motu* of the 1590s he rejects Copernicanism (Gal 1994).

asserts that it is not “an appropriate place to call into comparison with Tycho such men as Ptolemy and Copernicus” (Galileo “Assayer”; Drake and O’Malley 1960: 180). Thirdly, and most importantly, because Galileo’s famous paragraphs are part of a wholly empirical argument – the most curious attempt to discredit the value of parallax observations.

4. The price: Back to Aristotelianism

Taking a strong position against the use of parallax calculations, Galileo makes the traditional interpretation of *The Assayer* as an anti-Aristotelian manifesto completely untenable. This is by no means to underestimate the role that Galileo’s instrument played in the demise of Aristotelianism, a role acutely summarized by Van Helden (1974: 55):

Men who ... grew up with the telescope and were taught what it revealed formed a different fundamental conception of the cosmos from that of their predecessors. The Aristotelian duality between the sublunary and superlunary regions had been the commonsense notion for a long time: the only permanence was found in the heavens, and all bodies above the moon, with the exception of the sun, looked alike. They were all stars; some were fixed, and some wandered. But after the telescope had been directed to the heavens ... the planets were shown to have many things in common with the earth, while ... the fixed stars now appeared much farther away than had ever been dreamed before.

Galileo’s observations of the maculate moon and the satellites of Jupiter in 1609, the phases of Venus in 1610 and the spotted sun in 1612 were indeed devastating blows to the “Aristotelian duality between the sublunary and superlunary regions”. For practitioners of astronomy, however, they were only spectacular reiterations of the controversial claims that the new astronomy was making for some time. The enormous distance of the fixed stars was entailed by the Copernican Hypothesis, already 70 years in print and almost a hundred in circulation when the controversy began. The assault on the permanence of the heaven was the work of Tycho Brahe, almost 50 years before *The Assayer*.

This assault was fundamentally dependent on parallax calculations. No Copernican himself, Tycho, like his Jesuit disciples, was making empirical claims. With the unprecedented accuracy allowed by his very large, though essentially traditional sextants and quadrants, he could claim with authority that the *Stella Nova* of 1572 and the comet of 1577 were above the moon. He was thus placing ephemeral objects in the heavenly realm, where supposedly only “permanence was found”. The heavens, Tycho was arguing decades before the telescope, are changing (cf. Van Nouhuys 1998).

The use of parallax in astronomy was therefore not only the most established technique of mathematical empiricism, employed since Antiquity to make such discoveries as the size of the earth and the relative distances of planets; it was crucial for producing that “different fundamental conception of the cosmos” which was presumably Galileo’s primary intellectual motivation. If Galileo’s intention in the controversy on the comets was “a polemic against the Aristotelian and scholastic physics”, then Tycho should have been his close ally and parallax observations his cherished weapon. Yet early on in the controversy Galileo has Guiducci claiming on his behalf,

in order to have the comet appear as without parallax to all observers, and still originate in the elemental sphere, it would suffice for vapours ... to be diffused on high and to be capable of reflecting the sun’s light through distances and spaces equal to ... those from which the comet is perceived.

(Guiducci “Discourse”; Drake and O’Malley 1960: 40)

This is as removed from “opposition to the closed system of the schools” as one can imagine. Not only does Galileo reject parallax; and not only does he do it with a proto-Aristotelian concept of the comets and vapours; he does so in defending “the Aristotelian duality” between heavens and the “elemental sphere”. The positions Galileo takes in the controversy concerning its explicit subject matter – the nature and position of comets – are extremely conservative, not to say reactionary, and far more “Aristotelian” than those of his “opposition ... of the schools” – the Jesuits.

The special import of *The Assayer* in the historiography of science requires another short digression into existing interpretations of the arguments it comprises. Those who have noticed how reactionary Galileo’s position is tend to attribute it to the importance he ascribes to defending the original Copernican system against the Tychonic compromise (Drake and O’Malley 1960: xiii–xiv; also Shea 1972: 86–88). If comets are planet-like, Tycho’s system is indeed better adapted to accounting for their trajectory; it easily explains why they show no retrograde motion while in opposition to the sun, as the Copernican analysis will predict. But as Shea notes, this interpretation means that Galileo “was so intent on refuting Tycho that he failed to notice that he was pleading for a world where there would be no room for the heliocentric theory” (Shea 1972: 88); an inexcusable oversight over many years and the composition of two separate texts. Moreover, Kepler, no less a Copernican, had little difficulty in holding a super-lunar position for the comets, realizing exactly the significance of this discovery to the demolition of the old world view and the substantiating of the new. There were voices claiming that planetary analysis of comets means that “the Copernican hypothesis would collapse”, but even those were fully aware that this is so

on the condition that “other way of saving the appearances could not be found” (Ramponi, cited in Shea 1972: 86). If Galileo’s primary target was Aristotelian cosmology, he would have been much better served by searching for such “other way” than by adopting questionable Aristotelian objections against Tycho. Finally, interpreting Galileo as defending Copernican heliocentrism from the Tychonic compromise relies on a clear division between two aspects of the texts he produced in the controversy. On the one hand there supposed to stand the debate about the position of the comets, where Galileo maintains “a decadent Aristotelianism;” on the other, the debate about method, in which his position is supposedly a paradigm of “progress” (Shea 1972: 85). There is, however, nothing to support this division. Not only are the “decadence” and “progress” of the *Assayer* entirely intertwined; Galileo’s claims about the comets are wholly subservient to his attempt to refute Grassi’s argument.

Galileo, it needs stressing, does not have a strong position of his own as to what comets really are, and his trust in the Aristotelian theory is tentative and qualified at best. Relying on this theory to cast doubt on Grassi’s parallax calculations means replacing a mathematical-empirical technique with a qualitative and analogical mode of explanation, Aristotelian in essence if not fully Aristotelian in details. Together with the curious concept of friction causing cooling, this is in stark contradiction to his career-long commitments in both method and substance. It is also not the case that Galileo harboured some deep aversion to parallax considerations, or that he was always so committed to Aristotelian meteorology or convinced of the errors in Tycho’s way. In a series of public lectures following the super nova of 1604 he took a clear Tychonic position and used the lack of parallax to argue that the new star was super lunar (Galileo 1899–1909, II: 277–284; cf. Dupré 2003: 373). The steepest price, however, is in the very rejection of Tycho and Jesuits’ breakthrough claim that comets are “not sublunar but clearly celestial”. In declining to accept their demonstrations for the changes in the heavens and in employing the notion of “elemental sphere” on his own account, Galileo re-embraces the sharp distinction between the sub- and super-lunar realms.

5. The rival: The Jesuits’ mild instrumentalism

Galileo pays a heavy intellectual price for the positions he takes in the controversy and for reasons he never makes completely explicit. *The Assayer* clearly cannot be read as a simple confrontation of the new against the old, the mathematical against the verbal, or the empirical against the scholastic. This does not mean that it should no longer be thought of as a manifesto of the epistemology of the

New Science, but that the empiricism it argues for and the positions that it recognizes as rivals are completely different than what has been assumed, and that these intellectual motivations are significant enough for Galileo to suspend his anti-Aristotelianism.

These intellectual motivations reveal themselves in that famous sentence which constitutes the *Assayer's* claim as a manifesto of the New Science: "Philosophy is written in this grand book – I mean the universe ... in the language of mathematics". Yet they demand a new interpretation of this assertion, free of all common assumptions about Galileo's empiricism or Platonism. Galileo's plea for replacing the language of authority with the language of mathematics is not directed against some ancient authority – of Aristotle, of religion or of habit and commonsense. It is, rather, a direct assault on a particular type of empiricism, encapsulated in Grassi's knockdown parallax argument:

If at the same time from other regions the same star was also observed very near to the comet, no stronger and clearer argument could be hoped for by which it might be demonstrated that the comet had very little or no parallax, since this could be observed without any instrument and by *observation with the unaided eye*.

(Grassi "Disputation"; Drake and O'Malley 1960: 14; italics added)

What for Galileo is the core of the debate, what defines for him the Jesuit-Tychonic position on parallax, is captured in this last clause. This is what his rhetoric of mathematics, which has always fascinated historians and philosophers of science, is aimed to demolish and replace. The debate *is not* about dogmatism vs. empiricism and definitely not about whether instruments should be employed; it is not a case of "an open system of scientific inquiry in opposition to the closed system of the schools". It is a debate between two versions of empiricism, between what one may call Renaissance and Baroque concepts of directness and mediation, about the relative import of eye and instrument.

For Grassi and the Jesuits, the final arbitrator and the measure of all observations is the "unaided eye". Grassi is not at all opposed to the use of the telescope. In fact, he opens his *Disputatio* with a eulogy to the achievements of "the lynx eyes":

Now no part of the sky escapes our glance, nor is the beauty of the moon so great as it was for us formally. We have been able to distinguish the circular motions of Venus and Mercury, and who does not blush to see the sun occasionally disfigured? We have laid bare the stratagems of Mars in approaching the earth and we have exposed the attendance of Jupiter and Saturn, hitherto hidden away to no purpose.

(Grassi "Disputation"; Drake and O'Malley 1960: 5)

Commonsensically, Grassi perceives the telescope as a legitimate, indeed marvellous, extension of the eye, compensating for its weaknesses and repairing its errors. As Antoni Malet (2005) has recently shown, presenting the telescope as an aid to the eye was the common way of conceptualizing its novelty among its most sophisticated users and theorizers, including Kepler. Indeed, interpreting Galileo as debating for this legitimacy, Grassi bitterly protests (under the name of Sarsi and on behalf of his Jesuit colleagues) what he perceives to be his portrayal as a scientific reactionary. Nothing is farther from the truth, the author of *Libra* complains; he is a champion of progress and a staunch defender of Galileo and his instruments:

There were not lacking those who ... asserted that ... the telescope carries spectacles to the eyes and deludes the mind with various images; therefore it does not display genuinely and without deception even those things which we observe close at hand, much less those which are far removed from us, except it will show them bewitched and deformed. We ... publicly confuted the ignorance of those for whom this instrument was of no significance ... we hoped that by protecting from invidious calumnies this telescope ... we might therefore deserve well of [Galileo] rather than ill. (Sarsi "Balance"; Drake and O'Malley 1960:80–1)

There is no reason for Grassi to reject the telescope. Traditional mathematical optics provides him with a clear and trustworthy account of the principles of the instrument's operation:

Objects are enlarged by the telescope because these objects are carried from it to the eye under a greater angle than they are observed without this instrument ... according to optics, whatever things are observed under a larger angle seem larger ... (Sarsi "Balance"; Drake and O'Malley 1960:79)

This analysis also provides him with an explanation, again in terms taken directly from traditional eye-centred, Euclidean based optics, why the fixed stars, and presumably comets, should elude magnification:

Be the visible objective whatever it may, the more it is removed from the eye the smaller and smaller the angle at which it is seen ... thus, the angle of incidence of the images at the telescope scarcely vary after the objects have reached a very great distance, for then it is just as if all the rays fell perpendicularly on the lens. (Sarsi "Balance"; Drake and O'Malley 1960:82)

Note how 'modern' Grassi is, how well entrenched in the most contemporary cosmology: Copernican or Tychoonian, Grassi's world offers "very large distance" for the fixed stars. Yet in one fundamental respect he remains wholly committed to medieval optical epistemology, his enthusiasm with the telescope notwithstanding: the "visible object", for him, is seen *by the eye*. The telescope is of an altogether

different status than the eye; it is a part of the medium *through* which vision occurs, and subject to the same mathematical analysis. It helps like any instrument might – hence the title *Libra* – but it does not change the principal onus of evidence and argument. This lies, always, with what “could be observed without any instrument and by observation with the unaided eye”.

6. The supremacy of the instrument

Grassi, we saw, was taken aback by the vehemence of Galileo’s replies, and understood them as a defence of telescopic observation. But his baffled defence of his credentials in this respect was misplaced. Galileo had even less patience for his hearty support than for his mild criticism: “Sig. Sarsi, give up trying to exalt this instrument with this admirable new properties of yours unless you wish to throw it into utter disrepute” (Galileo “Assayer”; Drake and O’Malley 1960:209). His sarcasm aside, what is clear is that Galileo was not disturbed by Grassi’s empirical claims, but by his analysis and arguments, enough so that he felt compelled to reject both Grassi’s conclusions and his support, even at the price discussed above. Galileo would not allow that one can conclude about the distance of the comets from the failure of the telescope to magnify them, because he would not admit to this failure; the telescope magnifies regardless of distance:

If a surface of a ball seen through the telescope at a distance of half a mile increases a thousand times, then so will the moon’s disc increase a thousand times and no less; so will that of Jupiter and finally that of a fixed star.

(Galileo “Assayer”; Drake and O’Malley 1960: 220)

Galileo has no qualms about Grassi’s geometrical analysis of magnification “for objects seen naturally”. In that case, “the diminution of the angle is made in a continually greater ratio the more the object is removed” (Galileo “Assayer”; Drake and O’Malley 1960: 221). But Galileo has little respect for the way objects are “seen naturally”. What Grassi completely fails to comprehend is that Galileo no longer seeks justification or even explanation for the use of the telescope in reference to the eye. From Galileo’s point of view, it is not the telescope which is epistemologically problematic but the eye; the instrument should not aid or extend the sense organ, but replaces it altogether. Devoid of the telescope, Galileo proclaims, the naked eye is not a reliable source of knowledge: “The naked eye distinguishes none of these shapes [of the heavenly bodies] without the telescope” (Galileo “Assayer”; Drake and O’Malley 1960:321).

It will be a mistake to think of Galileo as defending the telescope or apologizing for its failure to magnify very distant objects. There is no such failure, he

insists: comets and fixed stars do not appear magnified *to the eye* because of a distortion produced by the eye and repaired by the telescope:

what we meant by saying the telescope ‘robs the stars of irradiation’ ... is that it operates upon the stars in such a way [as to circumvent] the irradiation which disturbs the naked eye and impedes precise perception.

(Galileo “Assayer”; Drake and O’Malley 1960: 324; see also Dupré 2003)

The eye introduces a spurious splendour around stars and comets, and this splendour makes them appear larger. This is not real magnification, of course: the body of the celestial objects remains invisible to the naked eye. Because the telescope removes the “irradiation”, the eye fails to notice that it has also magnified and made visible the celestial bodies themselves. This is not an *apologia* for the instrument. It is, rather, a charge against the eye, which errs twice: once in introducing the false irradiation and once in failing to notice the correction and magnification. Grassi’s claim that fixed stars and comets “suffered scarcely any enlargement” (Grassi “Disputation”; Drake and O’Malley 1960: 17) meant that they were not perceived as larger *by the eye*. But the eye, in *The Assayer*, is no longer the main point of reference for visual phenomena, and definitely not the final adjudicator of their trustworthiness. The communication with Grassi unravels because for Galileo the mediation of the instrument is neither another source of error and distortion nor merely a remedy for the weaknesses of the eye.

The reversal of epistemological hierarchy that Galileo is aiming at makes sense of his attitude towards the Jesuits’ claims. Grassi presents two main arguments for the superlunary position of the comets. The first is that fixed stars, like comets, are not magnified by the telescope, which implies that the telescope does not magnify *all* objects. The second is that the absence of parallax is *the* unassailable testimony for the great distance of comets, which implies the supremacy of what “could be observed without any instrument and by observation with the unaided eye”. It is these arguments, rather than the claims they support, that Galileo is so forcefully rejecting.

Galileo does not reject Grassi’s cosmological conclusions because he nurtures some deep-held belief in the sublunary nature of comets. Rather, he finds himself placing the comets in the “elemental sphere” because he is adamant about rejecting the implications of Grassi’s arguments. The controversy between Galileo and the Jesuits is not between “open system” and “closed system”, but between mild and radical instrumentalism. *The Assayer* is a defence of a brave new brand of empiricism, in which the instrument obviates and replaces the human sense.

7. Radical instrumentalism

The controversy over the comets was not the last exchange between mild and radical instrumentalism. In the 1670s Robert Hooke, the curator of experiments for the Royal Society, had taken Johannes Hevelius to task for not using telescopic sights (*dioptrae telescopicae*) in his astronomical observations. As was the case in the controversy between Galileo and the Jesuits, both sides were committed instrumentalists: Hooke was the most distinguished designer of scientific instruments of the time, boasting contraptions from Boyle's air pump, through the microscopes used for his marvellous illustration of the 1665 *Micrographia*, to the large-scale clock-controlled rotating sextant of his *Cutlerian Lectures* (Figure 2). Hevelius, for his part, designed and employed the largest telescopes to date, including a 60 and 140 feet pieces (Figure 3). Their debate, therefore, was not about the very use of artificial observation devices, but about the relative epistemic status of eye and instrument. Hevelius argued, quite convincingly, that the poor quality of the currently existing sights hampered, rather than assisted his observations. To Hooke, Hevelius was missing the point:

There is therefore one thing in *Hevelius* his instruments, that though they be never so large, never so accurately divided, of never so choice and convenient material, and never so tractable for use, with one of about two or three foot radius of mettall [sic] with *Tycho's* Sights and Diagonal Divisions, which is occasioned by the limited power of distinguishing by the naked eye.

(Hooke 1674: 5; italics original, underline added)

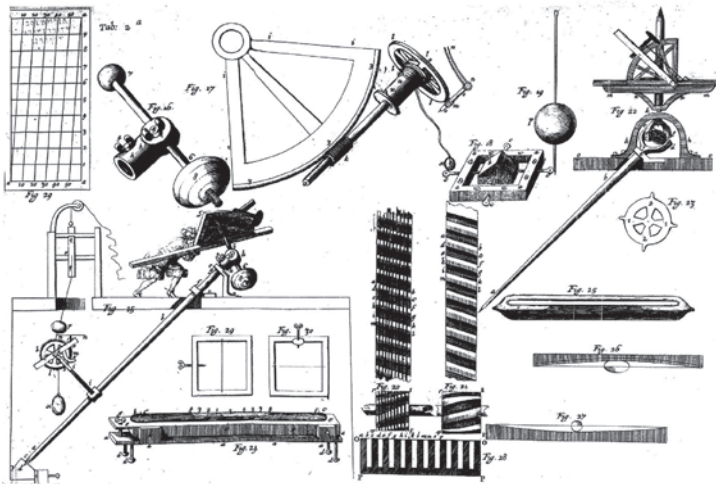


Figure 2. Hooke's pendulum-controlled quadrant (*Lectiones Cunlerianae*, 1679).

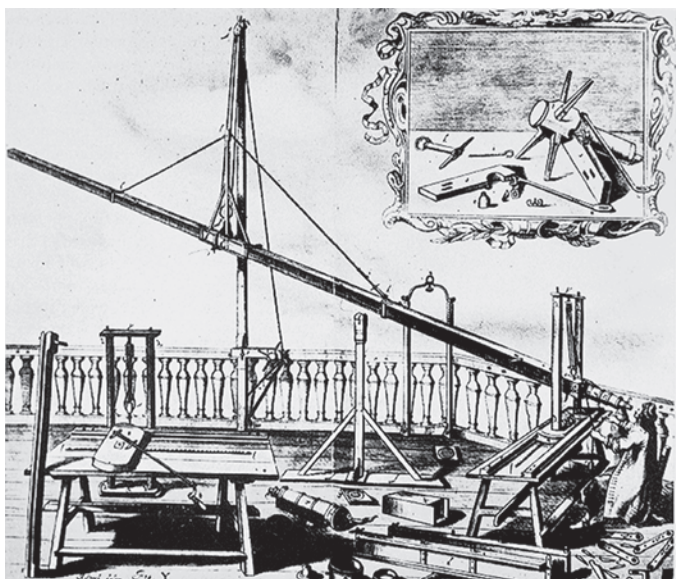


Figure 3. Hevelius' 60 feet telescope (*Machina Coelestis*, 1673).



Figure 4. Hevelius and one of his telescopes (*Selenographia*, 1647).

The respective illustrations neatly capture the difference in instrumental empiricism that Hooke's complaint expresses: his observer is immersed in the instruments, an undistinguishable, though unavoidable part of the optical process. Hevelius, on the other hand, personally warrants the quality of his instruments and observations; he is their very reference point, and it is his own portrait adorning all drawings of instruments. From the radical instrumentalist's point of view, Hevelius fails to comprehend the full significance of the scientific instrument: using the human sense as a reference point submits all observations to "the limited power of distinguishing by the naked eye". The instrument is always preferable, for it always presents the opportunity for further improvement. The eye is fundamentally limited; it should not be aided or augmented but replaced by what Hooke called his *Micrographia* "artificial Organs" (Hooke 1665: xi).

Nor was Galileo the first to deprive the eye of its privileged position in the optical process. Some fifteen years before the beginning of the comets controversy, Kepler (1604, 2000) published his optical *opus magnum*, the *Ad Vitellionem Paralipomena* in which he refashioned traditional mathematical optics as a theory of the production of images by light. Kepler's agenda in his *Ad Vitellionem* is similar to Galileo and Hooke's: to legitimize the use of *Artificiosa Observationes* – astronomical observations performed by means of artificial instruments. His strategy, carried to the full extent of its theoretical and philosophical ramifications, is also similar: rather than attempt to excuse the mediation of the instrument, Kepler abolishes the very possibility of unmediated vision. Establishing vision as a purely physical process in which light bounces off objects and on screens, Kepler removes all eye-directed entities that populated the ontology of optics – species and visual rays – and turns the eye itself into an instrument: "the pupil takes the place of the window"; the cornea is nothing but a lens; the retina nothing but a screen (Kepler 2000: 184). All vision is mediated in essence, so Kepler's argument goes, and since the eye is no better than the instrument, the instrument is no worse than the eye (see Gal and Chen-Morris 2010).

Kepler was fully aware of the far-reaching skeptical ramifications of his optics and was keen to hold them at bay, so much of the *Ad Vitellionem* is dedicated to demonstrating that the visual process, even though natural, causal and mediated, is trustworthy. Galileo, unperturbed by Kepler's careful optical and epistemological deliberations, is significantly more radical in his stand for the instrument: *the eye* mediates and distorts; the instrument provides the standard of trustworthy perception against which it is to be judged. In a sense, Galileo re-introduces the distinction between artificial and natural vision that Kepler laboured to abolish, reversing, as we said, the epistemological hierarchy. The two ways of observation, he is arguing, through the instrument and through the naked eye, provide data of entirely different value, not to be conflated or compared, and Grassi is fundamentally wrong to submit the telescope to the same analysis as ocular vision:

Your error lies in comparing the star taken together with its irradiation when seen with the naked eye to the body of the star alone when seen with the telescope and distinguished from the irradiated regions.

(Galileo "Assayer"; Drake and O'Malley 1960: 326)

Galileo, unlike Kepler, does not hesitate: his endorsement of the instrument comes at the expense and with the assistance of explicit distrust in the eye. The human organ is not merely weak but a positive source of various deceptions, which he makes a point to enumerate:

There is another illumination here, made by refraction in the moist surface of the eye, and by this, the real object appears to us to be surrounded by a luminous circle ... there is a third vivid splendor here, almost as bright as that of the original light itself; this is produced by reflection of the primary rays in the moisture at the edges of the eyelids, and it extends over the convexity of the pupil ... this radiant crown [is] a sensation of the eye ... it does not depend upon the illumination of the surrounding area.

(Galileo "Assayer"; Drake and O'Malley 1960: 319–320)

Like Kepler, Galileo thrusts the eye into the outside world. From a veridical conduit of knowledge it becomes part of the causal process of material nature, producing phenomena to be studied and explained physically. While the eye mediates, adding spurious and distorting brilliance, the telescope is not only a reliable source, but the standard against which to judge the observation made through the eye and the means by which to remove the errors it introduces:

Fancy to yourself some definite size for [a] wig, and in the center of this imagine a very tiny luminous body. The shape of this will be lost, being crowned by excessively long hair ... the telescope, by enlarging the star but not the wig, makes the tiny disc which originally was imperceptible ... so that its shape may be well distinguished.

(Galileo "Assayer"; Drake and O'Malley 1960: 322–323)

One can only speculate if and to what degree was Galileo deliberate in choosing the "wig" – this paradigm of artificiality – to denote the eye-added splendour. But his inversion of natural and artificial; direct and mediated; veridical and distorted is definitely deliberate, and Galileo provides it with a well-considered justification. The controversy over the place of the comets was, in truth, a struggle about turning vision inside out.

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Fair-mindedness versus sophistry in the Galileo affair

Two controversies for the price of one

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1. Introduction

The Scientific Revolution as a whole was a controversy. A crucial part of the Scientific Revolution was the Copernican Revolution, which was also a controversy. Similarly, within the Copernican Revolution, a key component was the Galileo affair, which was yet another controversy. By the Galileo affair I mean the sequence of events beginning with his telescopic discoveries of 1609–1610 and climaxing with his trial and condemnation by the Inquisition in 1633. By controversy I mean a disagreement where one side defends a particular proposition by means of reasons, arguments, and evidence, and the other side defends a contrary proposition by means of counter-arguments, counter-evidence, and contrary reasons; thus, the arguments or reasons for one side are counter-arguments or objections against the other. In the Galileo affair, the controversial proposition was the hypothesis that the earth moves.

My critical interpretation of the Galileo affair is the following (cf. Finocchiaro 1980; 1989; 1997; 2005c; 2008; 2010b). The Copernican Revolution required that the geokinetic hypothesis be supported not only with new arguments, but also with new evidence; and that it be not only supported constructively, but also critically defended from numerous objections based on astronomical observation, Aristotelian physics, scriptural passages, and traditional epistemology. This defense in turn required not only the destructive refutation but also the appreciative understanding of those objections in all their strength. Galileo's key contribution to the Copernican Revolution was a defense of Copernicanism that had such prerequisites: stressing reasoning and argumentation and judiciously guided by the ideals of fallibility, open-mindedness, and fair-mindedness. Nevertheless, Galileo's defense of Copernicanism was hindered by the Catholic Church. In fact, his trial can be interpreted as a series of ecclesiastic attempts to stop him from

defending Copernicus. In 1616, the Index decreed that the geokinetic doctrine was contrary to Scripture, which amounted to a general prohibition on defending Copernicanism from scriptural objections; and Cardinal Bellarmine warned Galileo to cease defending the earth's motion, which amounted to a personal prohibition on defending the Copernican hypothesis from the points of view of astronomy, physics, and philosophy. In 1633, the Inquisition condemned Galileo as a suspected heretic; this sentence amounted to condemning him for having defended Copernicus indirectly, implicitly, and probably in the *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632).

However, it would be naïve to pretend that my critical interpretation is not controversial. In fact, ever since Galileo's condemnation, another controversy has been raging about whether the Inquisition was right in condemning him. The central theme of this subsequent controversy is a series of arguments justifying his condemnation or defending the Church and counter-arguments defending him or criticizing the Church. My proposed resolution of the subsequent controversy is that Galileo can and should be defended from such ecclesiastic apologies or anti-Galilean criticisms by following the same approach which he himself followed in defending Copernicus. I believe there is merit in this proposal because it seems undeniable that he was successful in defending the Copernican doctrine, and so if we can model our own approach to the subsequent controversy on the Galilean approach to the original controversy, we stand a good chance of success. Of course, determining what Galileo's approach was is a nontrivial and theory-laden task, and after his approach has been ascertained its application must be judicious and cannot consist of mechanical, blind, or uncritical imitation.

The aim of this essay is to give a novel illustration¹ of the just-sketched critical interpretation of the original and subsequent Galileo affairs and their interrelationship. Historically, this illustration involves a figure and an episode that are relatively well known, but have been improperly interpreted and assessed. Philosophically, my illustration focuses on a method of argument that has been little studied by logicians, but has received some attention among scholars of critical thinking.

As will become apparent in due course, it is best to follow a roundabout manner of exposition. I want to begin with an item of Paduan history, in order to pay homage to the rich and illustrious history of the city and university of Padua. The item does not involve directly anything which Padua's most famous adopted son – I mean Galileo – did here during his eighteen-year (1592–1610) career at

1. Novel vis-à-vis the several examples used in Finocchiaro (2010b). A similarly novel illustration is found in Finocchiaro (2010a), involving the anti-Copernican argument based on the ship experiment and issues of epistemological and methodological apriorism and anarchism.

this university and residence in this city. Rather I am referring to an obscure item of Paduan history that happens to involve a native son of this great city. His name is Antonio Querenghi. However, as it will emerge in due course, we will quickly find our way back to Galileo, the affair, and controversies.²

2. Antonio Querenghi

Antonio Querenghi³ was born in Padua in 1546; received degrees from its university in civil and canon law and in theology; became a clergyman of the minor orders; and held various ecclesiastic, diplomatic, and secretarial positions in Padua, Modena, and Rome. In the late 1590's, Querenghi's house in Padua became a salon for the intelligentsia, and upon the death of Gian Vincenzo Pinelli in 1601, Querenghi became the leader in this kind of activity. His acquaintance with Galileo goes back at least to that period. In 1599, Querenghi helped to found the Accademia dei Ricovrati, and in 1604 he was elected its president. In 1607, he interceded in favor of Tommaso Campanella, who was imprisoned in Naples, and who wrote to him a very revealing letter. In 1609–1612, Querenghi served as a secretary at the court of the duke of Modena, Cesare D'Este (1552–1628). Querenghi died in Rome in September 1633, and was deeply mourned by Pope Urban VIII's Barberini family and household. From his deathbed, Querenghi is reported to have sent heartfelt greetings to Galileo, who had just been condemned by the Inquisition about two months earlier.

Querenghi published at least two books of poetry, one in Latin and another in Italian, and left unpublished many writings dealing with poetics and classic literary topics. Even more revealing I find the fact that his name is found in the works of others in a variety of contexts. For example, Querenghi was the recipient

2. I hasten to add that I introduce this Paduan character with some hesitation. For I am sensitive to the potential criticism that it is risky or rash (and perhaps even arrogant) for someone like myself who lives and works 10,000 kilometers from Padua to rummage through local history and hope to find anything new or insightful to tell. Still, let me take such risk and let me try to avoid any such imprudence.

3. I owe these details to documents and comments found in Galilei (1890–1909: vol. 2, 269–273; vol. 10, 116–142; vol. 15, 250–251; vol. 19, 437 and 514); Bolzoni (1980; 1981); Motta (1993; 1997); Vedova (1836); and *The Cardinals of the Holy Roman Church: Biographical Dictionary*, available at <http://www.fiu.edu/~mirandas/bios1599.htm> (consulted on 13 December 2007). There is some variation in this person's last name; Galileo scholars tend to follow Antonio Favaro, the chief editor of Galileo's works (Galilei 1890–1909) in naming him “Querengo”, but I shall use the spelling “Querenghi,” in deference to the most recent specialist scholarship, such as Bolzoni (1980; 1981) and Motta (1993; 1997).

of the dedication of an important document in the controversy over the nova of 1604, a dialogue on the topic, written in Paduan dialect, published in Padua the following year, to which Galileo himself contributed; this is the *Dialogo di Cecco di Ronchitti da Bruzene* (cf. Drake 1976: 34–36). Additionally, Querenghi was on friendly terms with Francesco Patrizi (1529–1597), so much so that the latter gave the name Querenghi to one of the interlocutors in his dialogue on the philosophy of love, *Amorosa filosofia*, written in 1577 and published only posthumously (cf. Bolzoni 1980: 141–142). Similarly, there is a character named Querenghi in the dialogue on the job of secretary published by Vincenzo Gramigna in 1620 and entitled *Il segretario* (cf. Bolzoni 1981: 162–169).

In 1615–1617, Querenghi was in Rome serving as secretary to Cardinal Alessandro D’Este (1568–1624), who was then residing in Modena and was the duke’s brother. Incidentally, Alessandro (a native of Ferrara) was an alumnus of the University of Padua. Thus, in 1615–1616, while Galileo was in Rome to defend the Copernican doctrine, Querenghi, as the Roman secretary of Cardinal D’Este, was in regular correspondence with the cardinal. Several of Querenghi’s letters reported on the content and atmosphere of those discussions.

3. Querenghi’s reports on Galileo in Rome in 1615–1616

Galileo arrived in Rome on 11 December 1615 (Galilei 1890–1909: vol. 12, 208–209), and despite the Christmas holidays, he lost no time in undertaking his defense of Copernicanism. By the end of the month, Querenghi could report to Cardinal D’Este that “we have here Galileo, who at meetings of men of curious intellect advances stupendous arguments about Copernicus’s opinion, which he regards as true” (Galilei 1890–1909: vol. 12, 212).⁴

In Querenghi’s letter of 20 January 1616, we have the most memorable and important description of the situation:

Your Most Illustrious Lordship would really like Galileo if you heard him argue, as he often does, surrounded by fifteen or twenty people who launch cruel attacks against him, now about one thing⁵ and now about another. But he is so well fortified that he is amused by them all; and although he does not persuade them,⁶

4. Here and elsewhere, the translations are my own, unless otherwise indicated.

5. Here I follow Motta (1993: 612) who transcribes this word as *cosa* rather than *casa*, which is the reading in Galilei (1890–1909: vol. 12, 226).

6. Note that his arguments are not “persuasive”.

on account of the novelty of his opinion, nevertheless he proves the invalidity of most⁷ arguments with which his enemies try to bring him down. On Monday in particular, at the house of Mr. Federico Ghisilieri, his arguments were astonishing; what I liked most was that, before answering the contrary reasons, he amplified and strengthened them with new grounds of great plausibility, so that after he destroyed those reasons, his opponents would appear more ridiculous.

(Galilei 1890–1909: vol. 12, 226–227)

A week later, Querenghi reported that Galileo was willing to go to Modena to personally explain his position to Cardinal D’Este. However, the discussions in Rome were continuing. In fact, Querenghi stated that he had just invited Galileo and several of his opponents to discuss the controversy at his own house over drinks (Galilei 1890–1909: vol. 12, 229).

4. Misinterpretations of Querenghi’s reports

As might be expected, there is a long history of misinterpretation of the Galilean technique stressed by Querenghi. This tradition includes illustrious scholars, such as Ludwig von Pastor, author of a monumental history of the popes in 40 volumes. In a volume on the papacy of Leo XI and of Paul V covering the years 1605–1621, Pastor includes an account of the first phase of Galileo’s trial in 1615–1616. Pastor’s view is the following:

It is certain that even in Rome there were those who admired the skill with which Galileo defended his views. He could marshal a whole row of plausible proofs in support of any opinion;⁸ if his hearers agreed he would knock down the whole structure with another set of arguments, thereby exposing to ridicule those who had previously fallen in with his views. But the theologians of the Inquisition were not to be impressed by such tricks.

(Pastor 1898–1953: vol. 25, 295)

Pastor’s interpretation, in turn, stems from an account advanced in 1909 by Adolf Müller, as Pastor’s own footnotes acknowledge. A direct inspection indicates that Pastor’s account is little more than an uncritical repetition of Müller’s, which reads as follows:

7. It is significant that the original text says *la maggior parte degli argomenti*; it does not say “all”.

8. Querenghi’s letters contain no evidence for the claim in this particular clause.

For an eloquent and versatile dialectician like Galileo, it was an easy thing to convey a high opinion of his knowledge to his friends and acquaintances, who often were mere amateurs in scientific questions. This petty desire to stand out led him occasionally even to defending an opinion and then, after the audience concurred, to “demonstrate” with equal skill the contrary. This was undoubtedly very entertaining for some, but it was out of place in such a serious controversy as the one at hand. Moreover, serious thinkers, who have never been scarce in Rome, did not allow themselves to be fooled by such tricks.

(translated from Müller 1911: 201; cf. Müller 1909: 151)

This anti-Galilean criticism, elaborated by Müller and authoritatively stamped by Pastor, was popularized by Arthur Koestler in his book *The Sleepwalkers* of 1959. Koestler added his own inimitable twist in the following passage:

I have quoted some samples of Galileo’s superb technique in his written polemics. According to his contemporaries, he was even more effective when he used “his tongue instead of his pen”. His method was to make a laughing stock of his opponent – in which he invariably succeeded, whether he happened to be in the right or in the wrong ... It was an excellent method to score a moment’s triumph, and make a lifelong enemy. It did not establish his own point, but it destroyed his opponent’s. Yet by the force of circumstances, these were the only tactics that he could adopt: to demonstrate the absurdity of Ptolemy’s epicycles and to pass in silence over the absurdity of Copernicus’ epicycles. (Koestler 1959: 452)

5. More careful critical analysis of Querenghi’s reports

Let us now try to elaborate a better understanding of this technique of Galileo and thereby defend him from such criticisms. For this purpose, it is useful to begin with the last letter on this subject from Querenghi to Cardinal D’Este. It was written on 5 March 1616, the same date when the anti-Copernican decree of the Index was published. This letter states that “Mr. Galileo’s disputes have dissolved into alchemical smoke, the Holy Office having declared that to defend that opinion is to clearly contradict the infallible dogmas of the Church” (Galilei 1890–1909: vol. 12, 243).

Now, today we know that such a declaration was not made by the Holy Office, but by its consultants in their confidential recommendation dated 24 February 1616; and that the Holy Office did not accept their recommendation in its entirety. Moreover, the only public declaration was not made by the Holy Office but by the Congregation of the Index, which issued a decree dated March 5. Thirdly, the Index’s decree did not declare that the geokinetic opinion was contrary to Catholic dogma, but that it was contrary to Scripture; clearly an additional step would

be needed to go from contrariety to Scripture to contrariety to the faith, and only the Church herself could take such a step with an explicit and public declaration addressed to all Catholics (cf. Garzend 1912; Beretta 1998:93–114); such a step was never taken. Perhaps Querenghi had not yet seen the Index's decree but had only heard rumors and indirect reports about what had been decided in the secret proceedings of the Inquisition and Index. If he had seen it, he was obviously misinterpreting it. My conclusion is that on the one hand this particular report is revealing regarding how a well connected clergyman like Querenghi perceived the situation, and so it is important as an indication of public perceptions; on the other hand, the report is inaccurate and thus it encourages us to adopt a critical attitude toward his other reports.

However, a critical attitude does not mean a totally and mindlessly negative attitude toward everything Querenghi says. Rather, it means, first, that his remarks must be compared and contrasted with other available evidence, namely weighed or evaluated vis-à-vis other evidence. Secondly, a critical attitude implies a discriminating attitude such that one distinguishes between what he can be taken to have observed directly and what he is advancing as an interpretation of his direct observations; and it is also important to distinguish between his interpretations and his evaluations.

For example, in his memorable letter of January 20, Querenghi reports that Galileo was in the habit of amplifying and strengthening his opponents' objections before refuting them. I take this assertion to be a description of something which Querenghi observed on numerous occasions. But then he adds that Galileo did this "so that his opponents would look more ridiculous". In this additional assertion, Querenghi is attributing to Galileo the intention of wanting to ridicule his opponents; and perhaps Querenghi is also speculating about the effect of Galileo's technique on the minds of the audience. In either case, Querenghi is advancing an interpretation, which may or may not be accepted. Finally, in regard to evaluation, it seems clear that Querenghi's attitude is positive and favorable (see Bolzoni 1980: 151); he expresses such an evaluation by means of such labels as "stupendous" and "astonishing" and by what seems to be a projection of his own emotional response of enjoyment onto the cardinal. But let us go back to the Galilean technique which Querenghi observed.

6. Deeper analysis of Querenghi's key point: Fair-mindedness

Let us examine more precisely what Querenghi is saying in his January 20 letter. First, he is clear that, despite what Müller and company say, Galileo was not being too successful at persuading his interlocutors; he was not convincing them

to accept the Copernican hypothesis of the earth's motion; or in less rhetorical and more logical terminology, we might say that Galileo was not succeeding in proving the geokinetic hypothesis. Second, it is equally clear that Galileo was convincing his listeners that the objections to the earth's motion were invalid or ineffective; that is, he was succeeding in refuting the objections against the earth's motion. However, thirdly, his success at refutation was not total and did not apply to all objections, but was partial insofar as he could only refute "most" objections; this crucial point is completely missed by Pastor and company. Fourthly, Galileo followed a very striking method of refutation: before responding to the anti-Copernican objections, he strengthened them.

That is, in his defense of Copernicanism, Galileo was operating on three levels: he was trying to provide positive or constructive evidence or reasons supporting the earth's motion; he was trying to articulate answers to or criticisms of the objections or counter-arguments; and he was trying to formulate the opposite arguments as strongly and plausibly as possible before refuting them.

This is a very sophisticated and powerful method of argument, but before I discuss further this evaluative judgment there is much more to be said about it by way of interpretation and analysis. This method has nothing to do with the way Galileo was depicted by Müller, Pastor, and Koestler, namely with the method of ridiculing opponents by means of the trick of first persuading them of one thing and then proving to them the opposite. On the contrary, the Galilean technique of strengthening objections before refuting them shows that the objections, although invalid, are serious, important, and plausible, and therefore that the opponents who believe the contrary thesis are reasonable people. Far from ridiculing opponents, this is a way of ennobling them, paying them respect, and enhancing their standing and credibility. Galileo's technique is, in fact, the anti-thesis of the widespread (then and now) practice of demonizing one's opponents, or differently expressed, it is the anti-dote to the straw-man fallacy.

Am I reading too much into Querenghi's January twenty remarks? For after all, like his report on the condemnation of the Copernican doctrine, they might be another example of his misrepresentation and misunderstanding of the situation. As suggested above, the evidence from Querenghi needs to be independently checked on the basis of other sources. However, there happens to be abundant confirming evidence. And it is important that such evidence comes from both Galileo's scientific practice and his reflective pronouncements, both from his deeds and his words, so to speak.

7. Galileo's reflective formulation of the fairness principle

Galileo's clearest and most relevant reflective pronouncement is the following general statement of principle: "when one presents arguments for the opposite side with the intention of confuting them, they must be explained in the fairest way and not be made out of straw to the disadvantage of the opponent" (Galilei 2008: 283; Finocchiaro 1989: 278; cf. Galilei 1890–1909: vol. 19, 343). I shall label this maxim the principle of fair-mindedness, or more simply the fairness principle, adopting this label from the literature on informal logic, critical thinking, and argumentation theory (see Ennis 1996: 171; Fisher 1991; Fisher and Scriven 1997: 90–91, 137–143; Paul 1990: 110, 111, 198; Scriven 1976: 166–167). However, the key idea and content of this principle has also been elaborated by many others under different terminology, for example by John Stuart Mill under the heading of freedom of thought, discussion, and argument, and the role of such freedom in the search for truth (Mill 1997: 52–84; cf. Finocchiaro 2005b; Finocchiaro 2007).

In this passage, Galileo was referring to what he had done in his 1632 book *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*. And I shall show in a moment how and why we can say that he practiced this principle in that book. But before I do that, more needs to be said about the context of this pronouncement.

In fact, the context is the very complicated one of the 1633 trial proceedings. In particular, Galileo asserted this sentence in the second deposition, dated 30 April 1633. This deposition was a confession of some wrongdoing, which he had denied at the first deposition on April 12. Between these two depositions, there was some out-of-court plea-bargaining between Galileo and the Inquisition's commissary. Galileo was now formally admitting that the *Dialogue* was a defense of Copernicanism; since he had never denied that the Church (through Cardinal Bellarmine's warning) had forbidden him to defend it, he was thereby pleading guilty to having violated this prohibition. This violation was unintentional because, Galileo also claimed, he had meant to refute Copernicanism, but by following the fairness principle, he had ended up giving the wrong impression. In this deposition Galileo was admitting violating Bellarmine's warning; he was explaining his violation as resulting from his anti-Copernican intention together with the fairness principle; and he was thereby justifying his violation as unintentional.

This was a courageous and pious confession of guilt, and an elegant and powerful denial of malicious intention. Nevertheless, it had a weak point. The weakness was not, however, the fairness principle, which once explicitly formulated is difficult to reject, and which in fact the Inquisition officials did not question. The weak point was Galileo's assertion of anti-Copernican intention in writing the *Dialogue*. In the judicial context of inquisitional procedure, the Inquisition could

and did proceed to a further test of the truthfulness of this assertion by interrogating the defendant under the threat of torture; and indeed on 21 June 1633 Galileo was subjected to such a so-called “rigorous examination”, which fortunately he passed with flying colors.⁹

However, in the present context we can reject the Inquisition’s kind of rigor, with its judicial fiction that torture or threat of torture can provide reliable evidence about intentions, and yet be more rigorous in another sense, namely insofar as we can judge an assertion on its own merits. Thus, we can simply reject Galileo’s assertion of an anti-Copernican intention as an understandable (and forgivable) misrepresentation designed to defend him in accordance with the Inquisition’s own rules. Instead, we assume that in his *Dialogue* he had a pro-Copernican intention, and then we are led to ask whether Galileo was complying with the fairness principle in his discussion of the anti-Copernican arguments. I believe the answer is resoundingly positive.

8. Galileo’s fair-minded practice: Venus objection

For example, consider the astronomical objection to Copernicanism based on the appearance of the planet Venus. In the version elaborated in the *Dialogue*, this argument amounts to the following (Galilei 1890–1909: vol. 7, 360–362; 1997: 236–237). Venus cannot revolve around the sun in an orbit just inside the earth’s own heliocentric orbit (as Copernicus claims) because if it did one should be able to observe two sets of phenomena, which are not in fact observed: first, Venus should appear much bigger when nearest to the earth than when farthest, specifically its disc about thirty-six times bigger and its apparent diameter about six times bigger; second, assuming Venus is inherently dark and shines only with light reflected from the sun, then it should exhibit phases so as to appear in a crescent shape when between the earth and the sun, in a full disk when on the other side of the sun from the earth, and in a semicircular shape at points about halfway between those two positions. The clarity and strength of this argument so formulated stems from the distinction between the two sets of observations required in a Copernican universe, and from the fact that it makes explicit the controversial assumption about Venus’s being intrinsically dark and not luminous.

Galileo’s refutation is that, although the two mentioned phenomena cannot be seen with the naked eye, they can be observed with the telescope, and such

9. This does not mean, however, that Galileo was actually tortured, as alleged by an old and persistent myth; for an explanation of how this myth originated and a refutation of its claims, see Finocchiaro (2009).

observations can be explained only by the hypothesis that Venus circles the sun together with the claim that it is intrinsically dark. Although such a refutation was effective and correct, it enabled opponents a clear and to some extent reasonable response, namely to deny the reliability of telescopic observations. In fact, this was the response made by a leading opponent of Galileo, Scipione Chiaramonti (1633: 55; cf. Palmieri 2001: 120–121). On the other hand, the reasonableness of this denial was to become smaller and smaller with the passage of time (cf. Chiaramonti 1644: 184; Palmieri 2001: *ibid*).

More generally, Galileo tried to portray this and the other observational astronomical objections to Copernicanism as unanswerable until and unless the telescope enabled astronomers to gather qualitatively new data. The other objections were based on such observations as the following: the insignificant variations in brightness and apparent size of the planet Mars; the fact that the moon revolves monthly around the earth; and the fact that heavenly bodies appear to undergo no physical changes, whereas terrestrial bodies are constantly changing. Galileo made his general point in a famous and controversial passage in the *Dialogue*, where, in my view, he expresses amazement (not approval) at Copernicus's ability to accept the earth's motion despite such insurmountable counter-arguments. Galileo's words are worth quoting:

We have already seen that the reasons against the earth's diurnal rotation, which have been examined, appear to be very good; the fact that they have been regarded as most conclusive by the Ptolemaics, Aristotelians, and all their followers, is a very good argument for their effectiveness. However, the observations that clearly contradict its annual motion appear to be even more powerful, so much so that (I repeat it) there is no end to my admiration of how in Aristarchus and Copernicus their reason could have done so much violence to their senses as to become, in opposition to the latter, mistress of their belief ... These are so clearly based on our sense experience that, if a higher and better sense than the common and natural ones had not joined with reason, I suspect that I too would have been much more recalcitrant against the Copernican system than I have been since a lamp clearer than usual has shed light on my path.

(Galilei 1997: 234–235; 1890–1909: vol. 7, 355–356)

9. Galileo's fair-minded practice: Extrusion objection

Another example of Galileo's fair-mindedness is provided by his discussion of one of the objections that were specifically against the earth's diurnal (rather than annual) motion and involved questions of physics (rather than astronomy), that is, the argument based on the extruding power of whirling (or centrifugal force,

in modern terminology). The refutation of this argument does not depend on the telescope, but on the physics of circular motion, which Galileo was in the process of elaborating. However, before refuting the objection, Galileo gives an explanation and clarification of the argument better than anything his opponents had come up with. Let us examine some details.

The anti-Copernican argument from the extruding power of whirling claimed the following. The basis of this objection to Copernicanism was the fact that in a rotating system, or in motion along a curve, bodies have a tendency to move away from the center of rotation or of the curve. Then the argument called attention to the fact that, if the earth rotates, bodies on its surface are traveling in circles around its axis at different speeds depending on the latitude, the greatest being about one thousand miles per hour at the equator. This sounds like a very high rate of speed, which would generate such a strong extruding tendency that all bodies would fly off the earth's surface, and the earth itself might disintegrate. Since this obviously does not happen, it was concluded that the earth must not be rotating.

Galileo's refutation of this argument consists of three criticisms. First, if a body were to be extruded from a rotating earth, the extrusion would occur along a tangent to the point of last contact with the terrestrial surface; the reason for this stems from the principle of inertia. But, because of gravity, on a rotating earth bodies would still have a tendency to move downward along the secant from the point of their position to the center of the earth. Thus, we need to do a comparison between these two tendencies; we cannot consider just the centrifugal extrusion, as the anti-Copernican argument seems to be doing. Now, the comparison shows that the downward tendency happens to be greater than the extruding one.

In the second criticism, Galileo tries to show that the downward tendency not only happens to exceed the tangential one, but it necessarily does so for mathematical reasons; that is, he argues that extrusion would be mathematically impossible on a rotating earth. He tries to prove this mathematical impossibility on the basis of the geometry of the situation in the neighborhood of the point of contact between a circle and a tangent, and the behavior of the external segments (called *exsecants*) of the secants drawn from the center of the circle to the tangent.

Galileo's third criticism is the following: it can be shown that in circular motion the extruding tendency increases with the linear speed but decreases with the radius; but on a rotating earth, the linear speed at the equator would be very small compared with the earth's radius; thus, on a rotating earth the extruding tendency would really be very small. The anti-Copernican objection ignores this aspect of the situation, and so it assumes that on a rotating earth extrusion would be more likely to happen than is the case.

Galileo's refutation of the extrusion objection is an attempt to show that this objection is quantitatively invalid, based on an analysis of the mathematical physics of terrestrial extrusion. When so reconstructed, Galileo's refutation is essentially correct, but not completely. This is especially true for his second criticism, which tries to prove the mathematical impossibility of extrusion on a rotating earth; and this proof contains parts that are mathematically valid but are misapplied to the physical situation, and parts that would be physically applicable but are mathematically incorrect.¹⁰ However, such details are beyond the scope of this essay.

The important point in the present context is that before refuting the extrusion argument, Galileo amplifies and strengthens it in several ways. One is that he gives some examples and evidence to establish the reality of the extruding power of whirling, which is a crucial premise of the argument. For example, he mentions the experiment of tying a small pail of water at the end of a string and whirling the pail in a vertical circle by the motion of one's hand; then suppose a small hole is made in the bottom of the pail; as the pail is whirled one will see water rushing out of the hole always in a direction away from one's hand. When in the *Dialogue* Galileo introduces these considerations, he does it with words that are uncannily reminiscent of Querenghi's account. Referring to the extrusion argument, he says that "I want to strengthen and tighten it further by showing even more sensibly how true it is that, when heavy bodies are rapidly turned around a motionless center, they acquire an impetus to move away from that center, even if they have a propensity to go toward it naturally" (Galilei 1997: 173; 1890–1909: vol. 7, 216).

Even more striking is that fact that Galileo begins his discussion with an essential clarification. He points out that as usually formulated, the argument is improperly stated. That is, its crucial step (namely, its main subargument) should be stated to say that if the earth were rotating then there would now be no loose bodies on its surface, since they would have all been extruded long ago due to the extruding power of whirling. Instead, the argument is usually misstated by claiming that if the earth were in rotation then we would see bodies on its surface extruded off toward the sky.

A third fair-minded point advanced by Galileo may be analyzed as a defense of the anti-Copernican extrusion argument from the unfair criticism that it commits the classic fallacy of *ignoratio elenchi* (cf. Aristotle, *On Sophistical Refutations* 167a21). The criticism would be that as ordinarily stated, the argument alleges to be proving one conclusion (that the earth is not in rotation) but instead at best

10. For more details, see for example Hill (1984), Finocchiaro (2003; 2010b:97–120), and Palmieri (2008).

proves another (that the earth did not recently begin to rotate); that is, the argument reaches an irrelevant conclusion, namely, a proposition that is not disputed. Such a criticism is unfair because once it is made, it becomes immediately obvious how the original argument should be stated and was intended to be stated.

These improvements in the extrusion argument are so many and so important that, together with the fact that it is hard to find it in the texts of Aristotle or Ptolemy, they have led at least one critic to claim that the argument was largely invented by Galileo so that he could refute it!¹¹ This claim is advanced as a criticism that exposes another one of the alleged tricks in Galileo's bag, there being allegedly something illegitimate and sophistical about formulating an argument against your own view and then showing that you can refute it. However, such criticism inverts the truth in the sense that it attempts to portray negatively a technique that is actually sound and valuable.

For in a controversial situation the formulation (and refutation) of arguments against your own position is another powerful method for the discovery of the truth. It is neither common nor easy, and certainly not for the faint-hearted; and it is usually a task to be left to one's opponents. But if one can pull it off, and if the logic of the situation warrants it, it provides another indication of where the truth lies insofar as our own position is being made to overcome an additional obstacle. In a sense, the technique of inventing counter-arguments to one's own position is one step further than the fairness principle; that is, if one has sufficiently strengthened and amplified an existing counter-argument, then at some point the degree of improvement is high enough that one is inclined to say that we are dealing with a new argument. Now, it is true that the extrusion argument one finds in Aristotle and Ptolemy bears little resemblance to the one reconstructed and refuted by Galileo. However, I think there is enough similarity to justify saying that he was strengthening an existing argument. More importantly, one should admit that it is a judgment call to draw the line between a greatly strengthened and amplified objection mentioned in the fairness principle and a novel one; but the second technique is very much in the spirit of the first.

11. This criticism is in Hill (1984: especially pp. 110–12). Galileo was presumably embellishing a version formulated by Copernicus (*On the Revolutions*, I, 7; 1992: 14–15). The latter was attributing the argument to Ptolemy. But the relevant passage from Ptolemy (*Almagest*, I, 7; 1952: 10–12) is insufficiently clear and explicit to determine whether he indeed had the extrusion objection in mind or was talking about terrestrial bodies being left behind if the whole earth were moving. For further clarifications about this aspect of the question, see Palmieri (2008).

10. Recapitulation and next step

I have been discussing a particular feature of Galileo's defense of the Copernican doctrine that was first noticed by the Paduan humanist Antonio Querenghi. The feature is the technique of amplifying and strengthening opposing arguments before refuting them.¹² I have argued that Querenghi was perceptive and accurate in attributing such a technique to Galileo, for there is considerable evidence (for example from the *Dialogue*) that Galileo practiced such a method of argument; and he was reflectively aware of the nature and value of this technique, as shown by his explicit formulation of the principle of fair-mindedness.

My account above has also defended Galileo from a common criticism based on this technique and advanced by such authors as Müller, Pastor, and Koestler. I have argued that their criticism misunderstands and misjudges this Galilean practice. Their misinterpretation is this: they describe this technique as the skill and practice of arguing in favor of anything one likes, both in favor and against a given proposition (as circumstances and whim might suggest, regardless of the truth and of one's honest opinion), and to do so in a persuasively successful manner. And then they add the assessment that such a technique is unscrupulous, deceptive, abusive, and manipulative. This anti-Galilean criticism may be labeled the sophistry objection to Galileo's defense of Copernicus; it is reminiscent of one of the charges against Socrates from which he defends himself in Plato's *Apology*; that is, the charge that Socrates was a sophist who practiced and taught the art of making the weaker arguments appear stronger.

Ideally, my defense of Galileo's defense of Copernicus in turn aims to comply with the fairness principle. That was one reason why I introduced into the discussion the views of Pastor, Müller, and Koestler. My lengthy quotations from their works were also designed as a step in the same direction, by avoiding oversimplifying or caricaturing their position. Full compliance with the fairness principle, however, would require more than I have done so far; it would require that I strengthen or amplify their arguments, a task to which I turn next.

12. Needless to say, however crucial and important, fair-mindedness is only one feature of Galileo's approach in his work in general, and in defense of Copernicanism in particular. For the identification of several other related attitudes (such as open-mindedness and judiciousness), see Finocchiaro (1980: 145–166; 1997: 339–344); and for some attempts at systematizations, see Finocchiaro (2005a: 34–45, 65–91, 386–430; 2010b: xiii–xliii, 121–134).

11. Strengthening the sophistry objection vs. Galileo

In fact, there is other evidence that some contemporaries of Galileo attributed to him what I have here called sophistical practices. One of these contemporaries is a relatively important figure: Cardinal Francesco Barberini, nephew of Pope Urban VIII, Vatican secretary of state during the trial of 1632–1633, and one of the trial judges as a member of the Congregation of the Holy Office, which was the tribunal trying Galileo. Cardinal Barberini's judgment is reported in a letter by Francesco Niccolini, the Tuscan ambassador in Rome, to Andrea Cioli, the Tuscan secretary of state in Florence. Here it should be recalled that in 1632–1633 the grand duke of Tuscany was treating Galileo's trial almost as an affair of state, partly because Galileo held the position of chief mathematician and philosopher to the grand duke of Tuscany, and partly because the *Dialogue*, the book that precipitated the trial, has been dedicated to the grand duke and had been published in Florence under his auspices. Thus, for about a year ambassador Niccolini was providing almost weekly updates about developments regarding Galileo's trial.

The relevant document is a letter dated 27 February 1633. Galileo had arrived in Rome from Florence two weeks earlier and had been allowed to lodge in Niccolini's residence at the embassy (Palazzo Firenze), instead of being locked in the Inquisition prison. The ambassador had had formal meetings with the pope and with Cardinal Barberini, and he had discussed with them the charges against Galileo and the plan for the trial proceedings. In this letter he was reporting to Florence about these discussions. In particular, he reported Cardinal Barberini saying about Galileo that "he knew how to express exquisitely and how to justify wonderfully whatever he wanted" (Finocchiaro 1989: 246; Galilei 1890–1909: vol. 15, 56). This judgment seems to confirm Pastor's claim, quoted earlier, that "he [Galileo] could marshal a whole row of plausible proofs in support of any opinion" (Pastor 1898–1953: vol. 25, 295). However, Pastor did not cite or mention this February 27 letter, but rather referred to Müller, who mentioned only Querenghi's January 20 letter, where we have seen there is no trace of this particular allegation. Thus, the sophistry objection can be strengthened in a way that goes beyond what its proponents themselves do.

However, as Galileo taught us, that does not mean that the objection is irrefutable. In fact, in this case the refutation happens to be in the same passage that reports Barberini's judgment. For in that same letter, ambassador Niccolini also reported some of what he himself had said in defense of Galileo, and when we take Niccolini's remarks into account it is difficult to deny that his point must prevail over the cardinal's. The relevant passage reads as follows:

I strongly recommended him to the protection of the Lord Cardinal Barberini, and I did this all the more gladly inasmuch as I felt I found His Holiness less irritated than usual. His Eminence replied that he felt warmly toward Mr. Galilei and regarded him as an exceptional man, but this subject is very delicate for it involves the possibility of introducing some imaginary dogma into the world, particularly into Florence where (as I know) the intellects are very subtle and curious, and especially by his reporting much more validly what favors the side of the earth's motion than what can be adduced for the other side. I said that perhaps the nature of the situation indicated this, and therefore he was not to blame; but His Eminence answered that I was aware that he knew how to express exquisitely and how to justify wonderfully whatever he wanted.

(Finocchiaro 1989: 246; Galilei 1890–1909: vol. 15, 56)

Niccolini is making the incontrovertible point that when one is discussing arguments for and against a given thesis (which is of course the norm in a controversy), one cannot artificially or falsely claim that the arguments on both sides are equally strong if those on one side are stronger. If the arguments in favor of the earth's motion were stronger than those in favor of the earth's rest, it was not Galileo's fault and he could not be blamed for pointing this out. Barberini's point seems to be that since Galileo had the ability of making the weaker argument appear stronger, he should have used that ability in writing the *Dialogue*. It's as if Galileo is being blamed for refusing to be a sophist!¹³

12. Additional strengthening of the sophistry objection

Let us now see whether the sophistry objection can be amplified or strengthened in some other way. It looks like it can insofar as it can be found in works that have better scholarly credentials than those of Pastor, Müller, and Koestler. In fact, with all due respect to Pastor, he was not a specialist in Galilean scholarship, but a papal historian, and so his views on the Galileo affair were second-hand and relied on specialists such as Müller. For the case of Müller, his work goes back about one hundred years, and the history of science in general and of Galilean scholarship

13. My thesis of a connection between this Galilean material and the concept of sophistry is further supported by a recent analysis of the connection between the problem of whether there are two sides to any issue and ancient Greek sophistry. On the latter, Jacqueline (2007: 125–126) has claimed that “what is objectionable is that ... the sophist argues pro and then argues con, and leaves it at that, omitting any assessment of the strengths and weaknesses of the arguments ... This is the sophists' error, that they defend a proposition, and then criticize it and defend its negation, as though the two were always in an even standoff”.

in particular have made considerable progress since then. The latter point applies to the fifty years since Koestler's work, as does the earlier point that he was not an expert, whatever his merit as a popularizer, as a novelist, and as an intellectual. Why not check what more recent and more professional scholarly works have to say about the sophistry objection? Now it so happens that here we come across a fourth Paduan connection in this story, besides that of Galileo's professorship at this university in 1592–1610, that of Querenghi's roots, and that of Cardinal D'Este's university education.

In fact, several years ago the University of Padua instituted the Galileo Galilei chair in the history of science, and this chair is presently¹⁴ held by a distinguished scholar who has made important contributions to Galilean scholarship, and more generally to the history of science (Shea 1972, 1991; Shea and Artigas 2003, 2006). Now, he too holds a version of the sophistry objection, and this is how he expresses it in a recent book. In the course of a discussion of Galileo's trip to Rome in 1615–1616, after a quotation of Querenghi's January 20 letter, we find the following critical interpretation:

Galileo's eloquence and his brilliant repartee made for great sport in the literary circles to which he was repeatedly invited, but the applause that he won had little to do with a genuine understanding of the nature of the argument. Most people enjoyed the liveliness of the discussion but treated the whole matter as a suitable topic for a debating society rather than a serious scientific enquiry.

(Shea and Artigas 2003: 77)

In an even more recent book, published in 2006, we find the word “one-upmanship” as a capsule description of this Galilean technique (Shea and Artigas 2006: 87). My point here is that what I call the sophistry objection is advanced not only in amateurish or outdated works on Galileo, but also in recently published professional works by distinguished specialists. To that extent this objection is thereby amplified and to some extent strengthened.

However, once again, amplified and strengthened does not mean valid and irrefutable. In fact, the objection stands refuted by my previous criticism. For I have already shown that Galileo did not preach or practice the art of making the weaker argument appear stronger; rather it was some of his opponents who would have liked him to use this art to make the geostatic theory appear stronger than the Copernican view. In particular, Querenghi's description in his January 20 letter of Galileo's *modus operandi* is not equivalent to this sophistical practice;

14. These words were written in the spring of 2008.

it is rather equivalent to being careful to avoid demonizing opponents or committing the straw-man fallacy, or, positively expressed, following carefully the principle of fair-mindedness. Galileo did defend Copernicanism in accordance with this principle not only in the houses of the Roman intelligentsia in the winter of 1615–1616 (as we have seen), and not only in the *Dialogue* of 1632 (as we have also seen), but generally and constantly at many other times and places.

13. Conclusion

In conclusion, what I have done in this essay is to discuss two controversies. The first is a seventeenth century controversy consisting of Galileo's defense of the Copernican doctrine of the earth's motion. The second is a controversy about this controversy, a meta-controversy so to speak, that began at least as early as Galileo's condemnation in 1633 and continues to our own day. The two controversies are in some ways very different: the original one was in the fields of astronomy and physics, and its key question was whether or not the earth moves; the meta-controversy is in the fields of history and epistemology, and its key question is whether or not Galileo's defense of the earth's motion was or is correct.

However, the two controversies have important similarities. First of all, they are both controversies. Furthermore, the second one is a controversy about the first, and so cannot avoid sharing at least indirectly many of the issues of the first. More importantly, my focus on the phenomenon of strengthening objections before refuting them has enabled us to see that the two controversies share at least two important features: a historical connection involving the city of Padua and the person of Querenghi, and the philosophical problem or concept of fair-mindedness. In my own argument that Galileo, when defending Copernicus, followed the fairness principle, I have attempted to follow this very principle. Or at least I have indicated how this principle could be followed. Or again, I have indicated how this principle could be followed at least in spirit, if not in its letter. For you may recall that I strengthened the sophistry objection after refuting it. However, I hope you will agree with me that this deviation from the letter of the fairness principle is more chronological than logical, in which case I may perhaps be said to have kept my promise to sell "two controversies for the price of one".

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PART II

Light and gravity

From cohesion to *pesanteur*

The origins of the 1669 debate on the causes of gravity

Victor D. Boantza

1. Introduction

The inaugural meeting of the French Royal Academy of Science took place on the last day of 1666. The discussion, recorded in a memoir entitled “de la recherche des principes des mixtes naturels” (AdS PV, vol. 1: 2–13)¹ was led by Samuel Cottureau Duclos (1598–1685), one of the Academy’s founder-members, its senior chymist, and the establisher of its laboratory and preliminary research agenda. During the winter of 1668–69 Duclos conducted a detailed series of lecture-demonstrations examining Boyle’s *Certain Physiological Essays*.² The Saturday meeting of 19 January 1669 marks the beginning of Duclos’ examination of the second and final half of the *Physiological Essays* – “The History of Fluidity and Firmness”. Duclos dedicated two consecutive memoirs (19/26 January) to the discussion of fluidity and one memoir (23 February) to the causes of solidity, cohesion, or “firmness”. The latter memoir was the final installment in a series of thirteen, comprising Duclos’ systematic scrutiny of Boyle, highlighting in particular issues in experimentalism and the mechanical philosophy, as applied to chymistry.³

1. Procès-Verbal de séance de l’Académie Royale des Sciences (hereafter AdS PV).

2. This is Duclos’ single most extensive critique of Boyle, comprising a systematic reading of the *Physiological Essays*, reported in 13 memoirs between September 1668 and February 1669. For details on this critique and for information on Duclos see Stroup 1990, 2002; Boantza 2007, 2010; Franckowiak 2009; Kim 2003: 48–52; Holmes 2003; Jacob 2006: 52–65.

3. I use ‘chymistry’ as synonymous with (al)chemistry, denoting the transitional phase (especially the sixteenth and seventeenth centuries) bridging classical and medieval alchemy with modern chemistry. See Newman and Principe 1998.

Boyle began the chapter by declaring that the “following Particulars touching Fluidity and Firmness were first written but by way of Annotations” upon the former “Essay on Salt-Petre” (the third of the five *Physiological Essays*);⁴ hence, he apologized, “the unaccurateness of the Method, as a fault scarce evitable on the occasion”. As for the content, Boyle claimed to have but “set down Experiments and other matters of fact related to the Subjects”, humbly inviting “abler Pens to contribute their Observations towards the compleating of what he is sensible he has but begun” (Boyle 1999–2000, vol. 2: 117). Possibly considering himself as an “abler Pen”, and in contrast to Boyle’s pretensions to scientific diffidence (Sargent 1995), Duclos related his general judgment of the whole treatise openly and early on, asserting the limitations of the “atomic or corpuscular doctrine” for explaining the phenomena at hand:

The history of fluidity and solidity, comprising the second treatise of this second part of Boyle’s book of essays, is presented as an imperfect and prudish draft, in order to make excusable that which is found less explicable and less consistent according to the atomic or corpuscular doctrine, which Mr. Boyle perhaps could not find as easy to establish upon chymical experiments as he would wish to suggest.⁵

(AdS PV, vol. 6: 14r)

Presented in the beginning of 1669, this critique triggered a lengthy series of implicitly and explicitly interlinked inquiries and controversies within the Academy, over the nature of matter, solidity, “firmness”, coagulation, cohesion, as well as terrestrial gravitation. From a general perspective, these series of disputes can be seen as dealing with the nature of ‘forces’ or ‘qualities’ that act upon matter at the micro- and macro-levels to produce the phenomena of fluidity, solidity, and cohesion. Boyle expressed this exact sentiment in his introduction of the subject at the outset of “The First Part. Of Fluidity”. Referring to “States” and “Qualities of Bodies” he noted that

4. Entitled *A Physico-Chymical Essay, Containing an Experiment with some Considerations touching the differing Parts and Redintegration of Salt-Petre*, part of what Boyle termed *Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy* (this broader title marked the section which includes the three essays on salt-petre, fluidity, and firmness).

5. “L’histoire de la fluidité et de la solidité qui faict le second traitté de cette seconde partie du livre des Essays de Mr. Boyle est produicte comme une esbauche imparfaicte et avec prudence, afin de pouvoir excuser ce qui se trouveroit moins expliqué et moins suivy selon la doctrine atomique ou corpusculaire que Mr. Boyle ne peut estre pas trouvée si facile a establir par les expériences chimiques comme il se l’etoit proposé.”

under which soever of the two Notions [states or qualities of bodies] we look upon them, 'tis manifest enough, that they are to be reckon'd amongst the most general Affections of the Conventions or Associations of several particles of matter into Bodies of any certain denomination, there being scarce any distinct portion of matter in the World that is not either Fluid, or else / Stable or Consistent. And therefore, I presume, it may be well worth while to consider what may be the general causes of these two States, Qualities, or Affections of Matter; and to Try whether by associating Chymical Experiments to Philosophical Notions, there may not be given at least a more Intelligible and more Practical account of both these Subjects, than has been hitherto afforded us by the Doctrine of the Schools, which is wont to appear very unsatisfactory to discerning Men.

(Boyle 1999–2000, vol. 2: 119)

Both Boyle and Duclos alluded to “Chymical Experiments” and “Philosophical Notions”. Yet the latter denounced Boyle’s claims to diffidence and stressed the shortcomings of corpuscular explanations in this context. Although typically less explicit in style, Boyle’s reference is unmistakable: these phenomena “are to be reckon'd amongst the most general Affections of the Conventions or Associations of several particles of matter into Bodies”. Given Boyle’s close and consistent association of mechanical explanations with standards of intelligibility, his general aim of providing “at least a more Intelligible” explanation becomes equally clear.

The series of interlinked controversies about the “general causes of these two States, Qualities, or Affections of Matter” spanned the better part of 1669 and climaxed eight to nine months later, when in the summer seven senior members of the Academy debated the causes of terrestrial gravity – *causes de la pesanteur*. During the same summer, at least four academicians participated in a debate over the causes of coagulation.⁶ Both controversies consisted in exchanges of memoirs delivered at the traditional weekly meetings of the Academy. The ‘philosophical group’ – in charge of natural philosophy, chemistry, anatomy, medicine, and botany – met regularly on Saturdays; the ‘mathematical group’ – dealing with geometry, astronomy, and mathematics – met on Wednesdays (Stroup 1990: 15; Sturdy 1995). The controversy on gravity lasted from August until late November and included contributions from five mathematicians and two natural philosophers. The debate on coagulation, which

6. The participants in the debate on gravity, in chronological order of memoir presentations, were Gilles Personne de Roberval (7 August and 4 September), Bernard Frénicle de Bessy (14 August), Jacques Buot (21 August), Christiaan Huygens (28 August and 23 October), Edme Mariotte (4 September and 13 November), Jean-Baptiste Du Hamel (6 November) and Claude Perrault (20 November); individual participants in the debate on coagulation included Duclos (9 March[?], 27 April, 1 June, 6 and 13 July, 14 December), Mariotte (20 July), Huygens (3 August), and Perrault (undated).

peaked in the same summer, originated in the beginning of the year and encompassed a lengthy experimental research program, attributed collectively in the minutes of the Academy's meetings to the "company", alongside individual contributions from one mathematician (Huygens), two naturalists (Edme Mariotte and Claude Perrault), and one chymist (Duclos).

The two controversies overlapped in both chronology and subject matter and shared disputants such as Huygens, Mariotte, and Perrault (see Bell 1947; Bos, Rudwick and Snelders 1980; Chareix 2006; Costabel 1986; Picon 1988). The controversy on the causes of gravity, at the confluence of kinematics, dynamics, and metaphysics, is a relatively well-known episode (Mouy 1934: 187–192; Dijksterhuis 1961: *passim*; Snelders 1989). The debate on the causes of coagulation and cohesion, which was prompted by Duclos' reading of Boyle, has been virtually overlooked by historians and philosophers of science. These controversies have never been considered together. Yet apart from evidencing and reconstructing the thematic-chronological connections between the two sets of debates, evaluating the two controversies against the background of prominent contemporary trends in matter theory, mechanics, chymistry, and their various manifestations, affords crucial insights into the processes of conceptual and empirical negotiation between mechanism and vitalism, and between the 'physical' and the 'chymical', as these spheres of knowledge were undergoing fundamental transformations during the last decades of the seventeenth century and the first decades of the eighteenth century. The period, the historical figures involved, and the themes included in these controversies are all intimately linked to what has come to be known as the Scientific Revolution, by now a controversial category in itself within the historiography of early modern science (see Introduction to this volume; Biagioli 1998).

2. The causes of gravity

On 7 August, senior mathematician Gilles Personne de Roberval (see Auger 1962) raised the general question concerning the possible causes of gravity. His introduction was followed by the memoirs of two other mathematicians, Bernard Frénicle de Bessy and Jacques Buot. On 28 August, Huygens relayed his theory of gravity in a lengthy account, which formed the blueprint for his *Treatise on the Causes of Gravity*, which was published in 1690 as an appendix to his *Traité de la lumière*. The following week Roberval and Mariotte raised a series of objections to Huygens' views in a joint memoir, to which the latter replied on 23 October, followed in November by the contributions of Duhamel, Mariotte, and Perrault. The debate on gravity turned, to a substantial extent, into a discussion and critical

evaluation of Huygens' strictly mechanistic theory, with the tripartite exchange between Roberval, Mariotte, and Huygens forming the controversy's centerpiece.

Introducing the issue, Roberval defined the gravity of a body as that which carries it naturally, without artificial aid, downward toward a center; one can thus consider, he noted, "lunar, solar, or jovial terrestrial gravities". Roberval distinguished between conceptions of gravity as a *primary cause* of motion and the view of gravity as a *consequence* of motion. As a primary cause, it might be either a quality inherent in all heavy bodies – thus possessed of 'heaviness' (*pesanteur*) – or it might be a common and reciprocal attractive relationship that exists between all gravitating bodies. On mechanistic accounts, motion causally precedes gravity, which is occasioned by a third (material) entity, usually a fast-moving subtle matter: an ether referred to as "corp très subtil". From the standpoint of the relationship between matter and activity, Roberval seemed to believe that matter is not essentially active in and of itself, and so he viewed gravity as a distinct principle although not necessarily physically separate. This stands in contradistinction to the scholastic notion of gravity as an intrinsic quality common to all heavy bodies. Roberval observed that despite the "manifest contrariety" between these three views, they also share common ground in that "they are [all] based solely on the pure thoughts and imaginations of their authors" and subsequently none of them consists of a certain proof. Aware of the difficulties involved in such debates, while remaining agnostic concerning the ultimate cause of gravity, Roberval concluded that if we allow for "occult qualities, that is, of which we have no real and specific understanding", the notion of gravity as a reciprocal attraction is "the most probable" one (Huygens 1888–1950, vol. 19:628–629).⁷

A devout mechanist, in a distinctly Cartesian vein, Huygens was the spokesman for the third alternative, according to which matter is universally homogeneous, inert, passive, and devoid of any inner activity. In terms reminiscent of Boyle's epistemological stance, Huygens claimed that an intelligible explanation

7. "J'appelle la pesanteur d'un corps ce qui porte ce corps a descendre vers un centre par la nature seule et sans artifice. Ainsi, on pourra considérer une pesanteur terrestre, une lunaire, une solaire, une joviale, etc ... ceux de la troisième [opinion] ont d'ordinaire recours a quelque corps très subtil qui se meut d'un mouvement très viste et qui s'insinue facilement entre les parties des autres corps plus grossiers, de sorte qu'en les pressant, il les pousse vers le bas ou vers le haut : et par ce moyen ils sont la pesanteur ou la légèreté. [...] Or, quoy qu'entre ces opinions il y ait une contrariété manifeste ; elles ont neantmoins cecy commun qu'elles sont fondées seulement sure les pures pensées et imaginations de leur auteurs qui n'ont aucun principe clair et évident ; et par conséquent ils n'ont aucune prévue certain de ce qu'ils disent sur ce subject. [...] Cependant si on suppose des qualitez occultes ; c'est a dire pour lesquelles nous n'avons point de sens propre et spécifique, cette seconde opinion me semble la plus vray semblable des trois". (Roberval's memoir, 7 August 1669).

of gravity must include nothing but matter and motion, a vision he referred to as the only possible “true and sound philosophy” (Snelders 1989: 212). Huygens’ theory of gravity was in effect a corrective variant of the Cartesian speculative hypothesis of vortices in which the tendency of bodies to fall was the consequence of an extraneous action, attributed to a *matière fluide* or *matière céleste* comprised of tiny rapidly moving particles, which fills up the space around the earth. The subtle matter – by definition the subtlest of all matter and hence capable of passing unhindered through the pores of all other material bodies – forms a whirlwind around the earth. Affected by the centrifugal tendency, the gravitational fluid is thrown back at the outer borders of the vortex, where it concentrates. Unable to keep up with the same velocity, coarser bodies that are found amidst the fast moving gravitational particles are inevitably pressed down towards the center of the earth. In other words, the greater *centrifugal* tendency of the subtle matter propels the coarser bodies *centripetally* (on Huygens and matter see Snelders 1980).

Roberval, Mariotte, and Frénicle favored by and large the dynamical explanation, considering gravity as a natural inclination, a quality causing material parts to join together. Huygens, Perrault, and Buot defended the kinematic-mechanical view, interpreting gravity by recourse to versions of kinematic corpuscularianism and speculative particulate theories. In line with late seventeenth-century proponents of mechanical explanations, Huygens highlighted intelligibility in his memoir (which was divided into eleven points). In order “to find an *intelligible cause* of gravity”, he proclaimed, “it is necessary to see how it can be done by postulating in nature only bodies made up of the same matter, in which no quality is considered, nor any inclination of approaching each other, but only the different sizes, shapes, and motions” (Huygens 1888–1950, vol. 19: 631; italics added).⁸

In their joint memoir Roberval and Mariotte responded to Huygens point by point. They began by charging him for excluding out of nature, “without proof, attractive and repulsive qualities” and for introducing, “without foundation, the sole sizes, figures and motions” of material particles. They further pointed out the circularity of his argument, since motion cannot be the sole cause of motion. In a search for real causality such a premise would require the inclusion of a primary cause, which would end up being “as incomprehensible as [scholastic, substantial,

8. Huygens wrote in his memoir dated 28 August 1669: “Pour chercher une cause intelligible de la pesanteur il faut voir comment il se peut faire, en ne supposant dans la nature que des corps faicts d’une mesme matière, dans lesquels on ne considère nulle qualité, ny inclination a s’approcher les uns des autres, mais seulement des différentes grandeurs, figures et mouvements”.

or occult] qualities, if not more so" (Huygens 1888–1950, vol. 19: 640–641).⁹ Responding in a spirit not unlike the one found in Newton's *Principia* about the cause of gravitation, Huygens insisted that "we know with certainty that moving bodies are capable of imparting motion to others, and that is all I require, without seeking here [to clarify] by which cause the primary motion had been introduced" (ibid: 643).¹⁰ Precluding the quest for an ultimate cause, Huygens limited the scope of his inquiry to *intelligibly causal* explanations, further stating that

I exclude from nature attractive and expulsive [repulsive] qualities because I am looking for an intelligible cause for gravity (*pesanteur*) since it seems to me that attributing the cause of why weighty bodies descent towards the earth to some attractive quality of the earth or of the bodies themselves, amounts to saying nothing. Yet as for the movement, figure and sizes of the bodies I do not see how one can say that I introduce them without foundation since our senses [and common sense] make it evident that these things exist in nature.¹¹

(Huygens 1888–1950, vol. 19: 642)

The accusations of circular reasoning, then, were mutual but not symmetrical. Huygens opposed the epistemological circularity entailed by assigning the cause of an observable and sensible (distinct and evident) attribute of heavy matter to an unobservable, indeed presupposed, quality of that matter. Deriving a sensible quality from an insensible one was gratuitous, unintelligible, and could subsequently form no real explanation. Roberval and Mariotte, on the other hand, were bothered by a different kind of circularity they identified in Huygens' argument, namely, the identical epistemological footing of *explanandum* and *explanans*, which from their standpoint denied the possibility

9. Huygens "veut qu'un mouvement soit causé par une autre mouvement, il faut donc venir a un premier qui est autant ou plus difficile a comprendre que les qualitez. Ne pouvant arriver a un premier mouvement par le mouvement mesme il en faut venir aux qualitez ou a une autre cause équivalente, et le mouvements qui en dépendront ne peuvent estre les siens comme il se verra cy après".

10. "nous sçavons certainement que des Corps qui sont en mouvement en peuvent mouvoir d'autres, et je ne veux que cela, sans chercher icy par quelle cause le premier mouvement a este introduit".

11. "j'exclus de la nature les qualitez attractives et expulsive parce que je cherche une cause intelligible de la pesanteur, car il me semble que ce seroit dire autant que rien du d'attribuer la cause pourquoy les corps pesant descendent vers la terre, a quelque qualité attractive de la terre ou de ces corps mesmes, mais pour le mouvement, la figure et les grandeurs des corps je ne vois pas comment on peut dire que je les introduicts sans fondement puisque les sens nous font connoistre que ces choses sont dans la nature."

of a genuine causal explanation. While mechanical attributes may be sensible or reasonably evident, and their mechanism of action imaginable and hence intelligible in some way, explaining motion by recourse to motion could not provide a proper causal account when motion itself requires a cause (Huygens 1888–1950, vol. 19: 640–643).

Roberval and Mariotte insisted that Huygens failed to avoid recourse to qualities. Huygens spoke of only two types of known natural motions, the rectilinear and the circular, the subtle matter being endowed with a circular motion. According to such reasoning, “as long as we consider only rectilinear motion and the [corresponding] reflections obtained between the parts of matter, we find nothing that guides them towards a center”. Hence gravitation must proceed from some effects of circular motion (Huygens 1888–1950, vol. 19: 631).¹² Yet, as Roberval and Mariotte argued, circular motion is in fact only a particular case of rectilinear motion, coupled with the influence of an additional quality: “circular motion in itself cannot be circular by virtue of motion alone; additional qualities or something equivalent must be involved”. In other words, circular motion is a mere consequence of rectilinear motion; it cannot be considered as either “natural” or primary and is hence not causative in this regard. Huygens countered this objection with the following statement: “I never claimed that circular motion is natural, but only that it exists in the world [in nature] which is an indubitable fact”, and concluded that “the tendency towards a center is therefore a constant effect of circular motion”. Roberval and Mariotte, by contrast, objected that this “conclusion is ... [not only] unwarranted” but is in fact “what we are looking for” in the first place (ibid: 641, 643).¹³

This charged exchange of views highlights the contemporary hostility towards so-called occult qualities and the dissatisfaction with the reductive-speculative features of the mechanical philosophy (see Gabbey 1985: 28–38).

12. “tant que l’on ne considère que le mouvement droit et les réflexions qui en arrivent entre les parties de la matière on ne trouve rien qui les détermine vers un centre”.

13. “le mouvement circulaire ne peut estre circulaire par le seul mouvement, et qu’il y faut de plus des qualitez ou quelques chose d’équivalent” (641); “je n’ay pas dict que le mouvement circulaire soit naturel mais qu’il y en a dans le monde, ce qui ne peut pas estre contredict” (643); “la tendance du centre est donc un effect constant du mouvement circulaire”; “la conclusion... ne semble pas nécessaire, et c’est qu’on cherche” (641).

3. The Newtonian context: Forces, big and small

Perhaps unsurprisingly, it is Newton that allows for one of the clearest expressions of these tensions and compromises. Many of the themes that preoccupied and shaped Newton's thinking on this subject, from the publication of the *Principia* (1687) until the later editions of the *Opticks* – the fourth and last edition of which was published three years after his death in 1727 – are vividly at play in the 1668–69 debates within the Academy. Newton had grappled throughout his life with the question concerning the relations between matter and activity, and between long-range and short-range forces. In the 28th query of his *Opticks* (4th ed.; 1730) he commended the ancient natural philosophers for considering “Vacuum, and Atoms, and the Gravity of Atoms, the first Principles of their Philosophy; tacitly attributing Gravity to some other Cause than dense Matter”. Much in line with the critique Roberval and Mariotte had leveled against Huygens and his theory of gravitation in 1669 (as we shall see), Newton complained that,

Later Philosophers banish the Considerations of such a Cause out of natural Philosophy, feigning Hypotheses for explaining all things mechanically, and referring other Causes to Metaphysics: Whereas the main Business of natural Philosophy is to argue from Phaenomena without feigning Hypotheses, and to deduce Causes from Effects, till we come to the very first Cause, which certainly is not mechanical.
(Newton 1952: 369)

By the 1720s, the last decade of his life, Newton felt more at ease in regarding restrictive mechanistic agendas, such as the one expounded by Huygens – who refused to include anything except matter and motion in natural philosophical accounts – as an instance of “feigning Hypotheses”. Newton's famous *hypothesis non fingo* made its first appearance in the controversial *General Scholium*,¹⁴ which was appended, by way of conclusion, to the second edition of the *Principia* (1713). Fashioned in part as an answer to Cartesian critics, it is there that we find Newton presenting an argument from design, suggesting that “No variation in things arises from blind metaphysical necessity, which must be the same always and everywhere” (Newton 1999: 942). Newton's distinction between sameness (causal sameness and regularity) and variety is evocative of his recognition, sometimes expressed only implicitly, of the multiple tensions between primary and secondary causes, mechanical and dynamical explanations, celestial and terrestrial phenomena, or gravitation and cohesion.

14. The *Scholium* first appeared in the 2nd edition of the *Principia* (1713) and was then appended, with slight changes, to the 3rd edition of 1726. For background on the *Scholium* see Newton 1999: 274–292.

The far-reaching religious dimensions of the *Scholium* notwithstanding, it is evident that Newton grew disaffected with strict and reductive mechanical principles, not only because of their theological implications, but also from a metaphysical perspective. The opening statement of the *Scholium* – “The hypothesis of vortices is beset with many difficulties” – bears witness to such tendencies, whereby “vortices” denote Cartesian vortex cosmology and “hypothesis” alludes to Descartes’ philosophical reasoning and method of rational inquiry (Newton 1999:939).¹⁵ The appearance and “motions of comets” formed one of Newton’s examples in this line of critique:

The motions of comets are extremely regular, observe the same laws as the motions of planets, and cannot be explained by vortices. Comets go with very eccentric motions into all parts of the heavens, which cannot happen unless vortices are eliminated. (Newton 1999:939)

Newton’s reference to comets in the context of “vortices” is not incidental; the precise nature of these occurrences has been at the center of numerous controversies and debates in the history of science (see Heidarzadeh 2008: 1–124). Aside from the astronomical-mathematical observations, measurements, and calculations involved in such debates, most of the controversies were undergirded by physical concerns, directly related to the ontological status of comets. A particularly pressing question was whether these bodies or occurrences were to be considered as celestial or terrestrial phenomena, and whether they subsequently belonged to the heavenly sphere – in which case their appearance would go against its alleged immutability – or whether they were part of the corruptible earthly realm. By 1726, the very foundation of this concern, largely dictated by the authority of the Aristotelian two-sphere cosmology, was gone; and from a mechanistic standpoint, Newton could state with certainty that the motions of comets “observe the same laws as the motions of planets”. Yet from a physical or matter theoretical standpoint the issue remained undecided, as expressed in Newton’s own speculation, found in the preceding paragraph, comprising the closing words of Book III of the *Principia*:

the vapors that arise from the sun and the fixed stars and the tails of comets can ... be condensed and converted into water and humid spirits, and then – by a slow heat – be transformed gradually into salts, sulphurs, tinctures, slime, mud, clay, sand, stones, corals, and other earthy substances. (Newton 1999:938)

15. Newton’s use of “hypothesis” is important and can be linked to his frontal attack on Descartes’ hypothetico-deductive method (found in the fifth paragraph of the *Scholium*).

Having admitted the inability “to deduce from phenomena the reason for these properties of gravity”, Newton proceeded to stress its empirical-phenomenal reality. “It is enough”, he stated, “that gravity really exists and acts according to the laws that we have set forth and is sufficient to explain all the motions of the heavenly bodies and of our sea” (Newton 1999: 943). Unlike Descartes, for whom God’s existence was axiomatic, Newton was particularly interested in understanding God’s agent in nature, the proximate cause for nature’s great diversity, life, and activity. He famously speculated about this cause in the concluding paragraph of the *Scholium*, proposing the action of

a certain very subtle spirit pervading gross bodies and lying hidden in them; by its force and actions, the particles of bodies attract one another at very small distances and cohere when they become contiguous; and electrical bodies act at greater distances, repelling as well as attracting neighboring corpuscles; and light is emitted ... and heats bodies; and all sensation is excited, and the limbs of animals move at command of the will. (Newton 1999: 943–944)

Newton distinguished between “gross bodies” and the “subtle spirit”, as the cause of cohesion and short-range attraction,¹⁶ on which account intra-corpuscular attraction and interaction was deemed to be closely related to vitality in nature.¹⁷ Although in the *Scholium* of 1713 (and in later versions) Newton still took great care not to speculate about what the cause of gravity *might* be, he was nonetheless quite explicit about what he thought it *could not* be:

this force arises from some cause that penetrates as far as the centre of the sun and planets ... and that acts not in proportion to the quantity of the *surfaces* of the particles on which it acts (as mechanical causes are wont to do) but in proportion to the quantity of *solid* matter. (Newton 1999: 943; italics in original)

Yet nearly four decades earlier, in the early 1670s, Newton argued in one of his alchemical tracts that “nature’s actions are either vegetable or purely mechanical [sic]”. In this context the mechanical was exemplified by the “operations of

16. For Newton’s views on matter and activity see McMullin 1978.

17. This is a highly complex issue. Many mechanical philosophers subscribed to a view according to which matter was inert and devoid of inner activity. In the alchemical and chymical traditions, the phenomena of cohesion and coagulation were closely related to processes of generation, fermentation, and putrefaction. In both cases, the question was what activates matter and how. Mechanical explanations tended to deal with this material ‘vitality’ by recourse to naturalistic explanations, avoiding appeal to supernatural notions. For an original and insightful study on this subject see Hutchison 1983. In the alchemical tradition, vitalism and matter were often connected through the concept of ‘semina’, seeds, seminal principles or archeus. On this subject see Hirai 2005; Henry 1986.

the vulgar chemistry ... as strange transmutations as those of nature ... that are but mechanically coalitions or separations of particles as may appear in that they returne into their former natures if reconjoined or (when unequally volatile) dis-severed, & that without any vegetation”, to which he added:

So far therefore as the same changes may bee [sic] wrought by the slight mutation of the textures of bodies in common chymistry & such like experiments may judge that such changes made by nature are done the same way that is by the sleighty transpositions of the grosser corpuscles, for upon their disposition only sensible qualities depend. But so far as by vegetation such changes are wrought as cannot be done without it, we must have recourse to some further cause. And this difference is vast & fundamental because nothing could ever yet be made without vegetation which nature useth to produce by it.

(Newton, Dibner Collection, MS 1031b:5r-v)

This contrast, which Newton drew between the “mechanical” and the vegetal realms on the one hand, and the corresponding practices of “vulgar” and ‘philosophical’ chymistry on the other, emerged as a central theme in the Academy’s 1669 debate over coagulation.

4. The causes of coagulation: Chymistry and mechanism

The controversy on the causes of coagulation climaxed during the months July-August of 1669, at which time most of the theoretical reports were presented: by Duclos, Mariotte, Huygens, and Perrault. The academicians’ interest in the subject, however, goes back to the beginning of the year, to Duclos’ critique of Boyle’s views on “Fluidity and Firmnesse”, the last communication of which was presented in late February. In March and then again in April of 1669, Duclos introduced the subject of “causes de la coagulation” and supported it by distinctly chymical interpretations and accounts (AdS PV, vol. 6: 60r–67r, 108r–117v, 199r–206r). These discussions raised great interest among the members of the ‘philosophical group’ who between April and July carried out a prolonged series of experiments on the subject. In early July Duclos took the helm again and recapitulated the subject.

During May and June, academicians recorded some one hundred and eighty individual observations, meticulously compiled and presented in half a dozen memoirs. Key observations in this ‘natural history of coagulation’ included the coagulation of milk, of egg white, of slats, oils, and of blood, both venous and arterial; in addition, two vivisections were performed and the pericardial fluids extracted from a horse’s heart were analyzed. The academicians carefully noted down the substances used, the reactions performed, and recorded diligently colors, smells, degrees of cohesion, and comparative viscosities obtained, among

other things. Much of this experimental endeavor was a joint effort and the respective memoirs were collectively ascribed to the “company”, but there were also individual contributions.¹⁸

The discussions, tensions, and contentions concerning the causes of coagulation take us deeper, as it were, into the world of micro-matter and the relations between macro phenomena and speculations about the micro-particulate realm. A consideration of the themes and the chronology, as evidenced even by a cursory glance at the run of the *procès-verbaux* for 1669, reveals clearly the continuity between Duclos’ critique of Boyle and the ensuing debate over coagulation. The nature of the link is instructive and merits a closer look.

In the opening statement of the essay “Of Firmnesse” Boyle argued that “It is sufficiently known that the Chymists ascribe the Firmness and Hardness of Bodies to Salt”,

and teach that the Saline Ingredient of them is the Principle of Coagulation in them, and the cause of their Compactness and Solidity. But though this opinion of the Chymists be embrac’d by so many modern Philosophers and Physitians, they some may think it superfluous to make enquiry after other Causes, yet others (to whom the Explications of Chymists seem not always so much as Intel-ligible) will upon the very account of the Receivedness of the propos’d Opinion, think it rather worthy to be examined than to be acquiesc’d in.

(Boyle 1999–2000, vol. 2: 150)

Duclos, however, clarified that

it is not the opinion of all the chymists that the salt is the cause of solidity in bodies, the most learned and experienced hold that it is the earthy sulfur, or rather that the ferments, which according to Van Helmont are the agents (*les auteurs*) of transmutations ... thus [for example] the coagulation and fixation of mercury into a perfect metal, by the elixir of the hermetics owes to the fermentative sulfur of that elixir, which the masters of the art call the sulfur of nature.¹⁹

(AdS PV, vol. 6: 46r–v)

According to common Paracelsian lore, as the most passive of the *tria prima*, which consisted of mercury, sulfur, and salt, the latter was indeed traditionally

18. Memoirs assigned collectively to the “company” were delivered on 4, 11 and 18 May; 1 and 15 June and 31 August. AdS PV, 6: 68r–84v; 89r–94v; 98r–107r; 173r–175r.

19. “ce n’est pas l’opinion de tous les chymistes que les sel soit la cause de la solidité des corps, les plus doctes et les plus experts tiennent que c’est le soulfre terrestre, ou plustost que ce sont les ferments lesquels comme dit Van Helmont sont les auteurs des transmutations ... ainsy la coagulation et fixation du vif argent en metal parfaits, par l’elixir des hermétiques vient du soulfre fermentatif de cet élixir, que les maistres de l’art appellent soulfre de nature.”

associated with the property of solidity and cohesion in bodies. Despite its allegedly sweeping popularity, Boyle deemed this perception as “rather worthy to be examined than to be acquiesced in”. Pointing, once more, to the unintelligibility of chymical explanations, Boyle recommended seeking “after other Causes”. Duclos, however, depicted a different state of affairs. Without denying that some practitioners subscribe to that view, Duclos stressed that “the most learned and experienced” chymists do not in fact attribute the solidity of bodies to the salt principle, but to the principle of sulfur, closely associated with activity within matter and especially transmutations or ‘deep’ (irreversible and non-mechanical) transformative chymical changes. Duclos’ references – to metallic transmutation, Van Helmont, “the hermetics”, the “masters of the art”, and most significantly, to “ferments ... [as] the agents of transmutations” – echo Newton’s chymical views and distinctions. What is more, Duclos’ words and allusions embed the same distinction between the “mechanical” and the vegetal realms, and between “vulgar” (Paracelsian-technical) and ‘philosophical’ chymistry, whereby the latter is associated with the “masters of the art”.

Duclos’ inaugural communication about the causes of coagulation consisted of a relatively brief memoir, dedicated to a disarmingly common and inconspicuous subject – “the concretion [coagulation] of milk” – a process that could be initiated in two different ways. “The concretions of milk”, Duclos observed, “the experiments upon which are common (*vulgaires*) and constant, are achieved either with or without the addition of some other substance”. He remarked that when placed “in a warm place” milk will curdle by itself; he proceeded to list a number of substances and extracts that would occasion the same effect once added to milk, such as rennet (*présure*), cotton thistle, the juice of safflower seeds, or various acids, which should be added to heated milk, like oil of vitriol (sulfuric acid), lemon juice, vinegar, white wine, or beer. For observing learnedly these milky coagulations Duclos advised paying particular attention to the presence of heat; the separation of its serum; the specific qualities of the added substances; and whether these shared any similarities (AdS PV, vol. 6: 60r–61r).²⁰

20. “Les concrétions du lait dont les expériences sont vulgaires et constantes se font ou sans addition au avec addition d’autre matière. Le lait de soy seul et sans addition d’autre matière se prend et se caille s’il est tenu en quelque lieu chaud. Les choses adioustées au lait, pour en faciliter la concrétion, sont ou la pressure [sic ; présure] ou la chardonnnette qui est l’espine blanche sauvage [onopordum acanthium] ... ou le suc de la graine de carthame [carthamus tinctorius]. Les acides meslez avec le lait cauffé le font aussi cailler comme l’esprit de sel commun, l’huyle de vitriol, le jus de citron, le vinaigre, le vin blanc, la bière, &c”.

On 27 April Duclos resumed his examination, delving further into the subject and providing a preliminary theoretical account. The memoir in question began with a brief presentation of the effects of various added substances: whether they facilitate the coagulation of milk, impede it, or have no observable function in this respect, and to what extent. “In all these coagulations”, Duclos surmised, “we find two different substances separated from each other, the curd (*caillé*) and the serum (*liqueur sereuse*). The curd does not seem to be a new product, since it is obtained from cow’s milk when left in a warm place without the addition of any substance”. Duclos concluded that the process at hand is one of *separation* between liquid and solid parts. The liquid is the milk’s serum; as for the solid parts, some are more sulfurous, others more earthy. In line with the traditional connection between the principle of sulfur and the chymical quality of oiliness and inflammability, Duclos argued that butter is the most sulfurous part whereas the curd is earthier (also observably more solid than butter). Thus the curdling of milk was nothing but a separation based on heat and the motion of non descript particles: “this separation is brought about by heat alone, which facilitates the movement of the parts of milk, by which movement each particle separates from those that are heterogeneous and unites to those that are homogeneous” (AdS PV, vol. 6: 62v–64r).²¹ Duclos further concluded that these processes are merely

condensations of solid particles of milk [preexistent; dispersed in the serum] ... these are not, therefore, transmutational coagulations; because transmutational coagulations consist of conversions and changes of liquids, simply into solids, with alterations of substance [substantial changes], changes that do not occur in condensative coagulations of milk, the so-called curd of which is thus the product of reunions of solid particles, which were discontinued, rarefied, and diffused within the serum.²² (AdS PV, vol. 6: 64r–v)

21. “En toutes ces coagulations du lait de vache il s’est trouvé deux matières différentes séparées l’une de l’autre, le caillé et la liqueur sereuse. Le caillé ne semble pas estre un nouveau produit, puisqu’il s’en trouve sans aide d’aucune autre matière ... Cette séparation se fait par la seule chaleur qui facilite le mouvement des parties du lait, par lequel mouvement chaque particule se sépare de celle qui luy sont hétérogènes et se joint aux homogènes”.

22. “ces coagulations du lait de vache ne sont donc proprement que des condensations des particules solides du dict lait, auparavant dispersées et délayées dans la liqueur sereuse. Ce ne sont pas des coagulations transmutatives ; car les coagulations transmutative, sont de conversions et changements des choses liquides, et simplement telles en solides, avec altération de substance lesquels changements ne se font point dans les coagulations condensatives du lait dont le caillé ainsy nommé ne se fait que par la réunion des particules solides, qui estoient discontinuées raréfiées et diffusées dans le serum.”

As I have shown elsewhere, this type of explanation squares well with Duclos' general view of the chymical action of heat, most prominent in the practice of distillation or fire analysis (Boantzsa 2007, 2010). It is important to note that Duclos' explanation includes *only* particles and motion generated by heat; the explanation is based solely on the differing propensities of the particles to move, which is taken as the standard of material homogeneity and heterogeneity in this context. As a particulate explanation, unlike common mechanical explanations, it precludes all reference to the *imagined* (imperceptible) shapes, sizes, textures, or specific patterns of motion of the particles in question. The separation occurs only in virtue of the two types of particles' (liquid and solid) differential tendencies to move. Reasoning analogically, Duclos speculated that a similar mechanism acts when these types of condensative coagulations are occasioned not by heat but by the addition of other substances. Acrid materials, for instance, are said to initiate a great degree of particulate motion whereas astringent substances help bring the particles together (AdS PV, vol. 6:65r–v).

Yet by the time he resumed his examination in early July, following the 'natural history of coagulation' conducted by the academicians, Duclos assumed a different approach and presented a fundamentally different account. This time, he began by considering and defining the nature of fluidity and solidity. For a chymical mechanist like Boyle fluidity depended upon three principles. First, the corpuscles must be small, round and smooth; second, their disposition must be such as to leave empty spaces between them, to enable their motion; third, and most important according to Boyle, the corpuscles must be all separately agitated and independently endowed with motion, "whether by their own innate and inherent motion, or by some thinner substance that tumbles them about in its passage through them" (Boyle 1999–2000, vol. 2: 122, 128, 130). A liquid, then, consists of small, round and smooth particles that continuously move, vibrate, and exchange places in space.

On Duclos' account, at once more general and more specific, a fluid consists of that which "cannot be retained within its own limits, and which can *flow without* the discontinuation of its particles"; this should be distinguished from "a heap of sand", which "*flows with* [by way of] a discontinuation of its parts". Finally, a solid is defined as an entity "the continuous parts of which are retained within the limits of their mass". Duclos considered the transition from liquid into solid as a "change of consistency" and referred to it as a "concretion". This was followed by a new and significant distinction, between two types of concretions: the congelative and the coagulative. In the former, the liquid solidifies by exposure to "cold and can return to its previous liquid state when exposed to heat, *without having its essence altered*". Within coagulative concretions Duclos

identified two sub-types: “coagulation is either of liquid as liquid or of solid particles ... mixed and diffused within some liquor”. The latter case refers to the process Duclos had previously considered – of separation between liquid and solid particles – which he termed “condensative coagulation” (AdS PV, vol. 6: 108r–109r; italics added).

Whereas initially Duclos viewed this type of process as a merely *physical* separation, he now argued that

in this change of consistency, there is also a *change of composition*. The milk curdles by condensation of its cheesy parts (*parties fromagère*), which separate from the serous liquid. And by this change of composition it ceases to be milk ... In the coagulation of liquid as a liquid, whether it is simple or a mixture of dispersed solid particles, the change in consistency entails an *essential conversion* of all or part of the liquid particles. And this coagulation is transmutational, and may be considered as a *transmutation* ... hence the mercury is changed into solid and durable metal by the chymists, the sap of trees becomes wood, and the chyle present in foods assumes in animals the solidity of their limbs [and organs].²³

(AdS PV, vol. 6: 109r–v; italics added)

It is the irreversibility of the process and the essential changes it entailed that distinguish it from the merely *physical* – ‘superficial’ and hence reversible – concretions, defining it as a ‘deep’-level, transmutational, *chymical* process.

Such explanations, involving particular substances, and relying on distinctions derived from various hands-on practices could hardly satisfy Duclos’ more ‘physically’ minded contemporaries. A thoroughgoing mechanist, Huygens for his part attempted to penetrate the domain of micro-matter with the same tools and categories he employed in his account of terrestrial gravity, highlighting, above all else, epistemological intelligibility. For Huygens, “coagulation produced consistent matter out of liquid”. “I believe”, he argued, “that for examining the cause of coagulation, we must first find out what comprises a liquid and what comprises a solid [consistent matter]”. Unsurprisingly, his definition of a liquid depended

23. “en ce changement de composition il se fait aussi changement de composition. Le lait se caille par condensation de ses parties fromagère qui se séparent de la liqueur séreuse. Et par ce changement de composition ce n’est plus du lait ... En la coagulation du liquide en tant que liquide, soit simple soit composé et mêlé de particules solides et délayées, avec ce changement de consistance il se fait conversion essentielle de toutes ou de quelques unes des particules liquides. Et cette coagulation étant transmutative peut être dictée transmutation ... ainsi le vif argent se change en métal solide et dur par le chymistes, ainsi la sève des arbres dévient bois et le chyle des aliments prend dans les animaux la solidité de leur membres.”

on the detachment of corpuscles and their perpetual motion. Arguing once more for the necessity of motion, Huygens averred that the natural tendency of liquids to form flat surfaces – assuming the lowest possible resting place – could not be attributed only to the sizes and shapes of their corpuscles. By way of example he suggested that a heap of sand will retain a pyramid-like shape and will not flatten down, like a liquid would, until it is externally shaken or moved. As to the alleged continuous motion, Huygens explained that since the particles are heavy (possessed of *pesanteur*; affected by gravity), they could not possibly conserve their own motion (AdS PV, vol. 6: 136r–137v).

On these grounds Huygens deduced the existence of a super subtle matter found in a state of rapid motion: it is all-pervasive and forms the cause of the particles' motion and corresponding state of fluidity. Huygens deduced the existence of the ether from the gravity assigned to the particles. Once more, the circularity in his reasoning is evident: as the cause of *pesanteur*, the ether denied the particles from conserving their motion; as the cause of fluidity, however, it provided them with the necessary continuous motion, which in fact constituted the liquid. In his account of fluidity, Huygens used the ether ambiguously as both cause and effect. Coagulation and cohesion, he reasoned, were due to the loss of the motion of particles. Unlike Descartes, however, who assigned cohesion to the lack of corpuscular motion, Huygens, perhaps influenced by Gassendi (see Millington 1945), maintained that

material consistency is nothing but the privation of the motion of the particles, which is the cause of some kind of attachment between them. This attachment derives, I believe, from the shape of the particles which enables them to hook onto each other and bind together; I am not of the same opinion as Mr. Descartes, who held that the lack of motion of the particles, resting upon each other, is enough to render a body very solid.²⁴ (AdS PV, vol. 6: 138r)

Mariotte and Perrault expounded similar theories, referring to corpuscular texture (AdS PV, vol. 6: 126r–135r, 141r–149v). The recourse to the sizes and especially to the shapes and configurations of particles was a widely applied practice in mechanical conceptions of matter. And while Huygens, it may be argued, was not

24. “la consistance des matières n'est autre chose que la privation du mouvement des parties a raison de quelque attachement des unes aux autres. Cet attachement vient a mon advis de la figure des parties qui ont des accroches pour se prendre et lier ensemble ; car le ne suis pas en cela de l'advis de Mr. Des Cartes, qui veut que le repos seul des parties, les unes auprès des autres suffise pour composer les corps les plus durs.”

much of a chymist, Boyle is a good case in point, since corpuscular configuration and texture played a significant role in his matter theory.

In his essay on fluidity, Boyle repeatedly explained various aspects and changes in the cohesion (or state of cohesiveness) of substances – set in chemical processes – by turning to corpuscular motions, sizes, shapes, and configurations. The fluidity of viscous egg whites, for instance, is increased by whisking them, an effect which “seems to be produced but by pulling asunder the parts, (which perhaps before were long and somewhat twin’d) and breaking them into shorter or lesser, and consequently more voluble ones” (Boyle 1999–2000, vol. 2: 127). Likewise, the main difference “between solid ice and water, [is] that in the one the parts ... [may have a] newly acquir’d texture” (ibid.: 130). Pressure, Boyle conjectured, may also have similar effects, as in snow, which “in first falling, is of an open and loose texture”. By compressing it between our hands we change the order of particles, bringing them into a closer order, producing small consistent (solid) icy bodies. Boyle even went as far as to interpret the action of the universal solvent, the alkahest, along mechanical lines. In a statement reminiscent of Newton’s reference to nature’s “variety” Boyle suggested that

if that be true which Helmont in several places affirms of his prodigious liquor, Alkahest, it is possible to turn Plants, Animals, Stones, Minerals, Metals, or whatever kind you please of consistent Body here below, into a Liquor equiponderant to the resolv’d concrete: which (if granted) seems to argue, That the most solid Body by being divided into parts small enough to be put into motion ... may become fluid.

(Boyle 1999–2000, vol. 2: 127–128)

The way Duclos interpreted these lines is revealing. First, he remarked that the agitating action of the whisk could have no effect on the shape of the particles or make them any rounder. Secondly, even if the compression of the alleged particles of snow makes the mass sensibly harder, it does not follow that all solidity is occasioned by compression and the restriction of intra-corpuscular space. Ice is solid yet its particles are less compact; it is more extended and less heavy than liquid water. Finally, fluidity of metals dissolved in strong acids proceeds from the “discontinuation” of their particles. If not radically resolved, these particles could reunite and regain their metallic solidity and properties. Fluidity caused by the alkahest, however, proceeds from a completely different cause since the liquors involved are unrecoverable (AdS PV, vol. 6: 15v–16r). Duclos attributed motion, for instance, to what he termed a “symbole” between two metals, a difference in salts, or a concentration of earthy fatness by saline materials. Regarding metallic dissolutions, he distinguished between what can be considered a physico-mechanical decomposition and a chymical one:

The fluidity acquired by metals in acids (eaux fortes) that dissolve them may well follow from the discontinuation of the particles of their bodies, which cannot be radically resolved by such means, and which may be, by way of union, restored to these bodies their primary metallic solidity. But the fluidity of bodies [that are] radically and completely resolved by means of Van Helmont's alkahest, must proceed from some other cause than the discontinuation of the reduced particles, since these liquors are irreducible [irreversible; irrecoverable].²⁵ (AdS PV, vol. 6: 16r)

Duclos, as we have previously seen, was not against a particulate approach, nor was he denying the influence of corpuscular motion. Yet, like in his critique of Boyle's explanation of the action of niter, Duclos rejected all mechanical reliance upon what he considered as the presupposed and imaginary sizes, shapes, configurations, and textures of particles. Most importantly, unlike Boyle, whose project was a physico-chymical one – an attempt to reconcile the chymical with the physico-mechanical – Duclos, like Newton, sought to set the chymical apart from the physical and hence resist both the structural reduction as well as the dogmatic denial of action at a distance.²⁶ Rather, Duclos claimed the independence of both realms, a view that is evident in his interpretation of fluidity, coagulation, and cohesion.

25. "La fluidité que les métaux acquièrent dans les eaux fortes qui les ont dissoutes peut bien proceder de la discontinuation des particules de ces corps, qui ne sont point par ce moyen radicalement résout, et lesquels, peuvent par leur réunion redonner a ces corps leur premier solidité métallique. Mais la fluidité des corps radicalement et totalement résout par l'alchaliest de Vanhelmot doit proceder de quelque autre cause que de la discontinuation des particules atténuées, puisque ces liqueurs sont irréductibles."

26. A certain degree of ambiguity can be ascribed to Newton's views on this matter. But it seems to owe largely to a discrepancy between his published material on the subject (most explicitly found in the *Opticks*) and his alchemical tracts. The famous 31st query (*Opticks*, 4th ed., 1730) may be interpreted as implying what McMullin referred to as a "one-level ontology of forces, regarded as a species of active principle". It could be argued, in this context, that Newton suggested a kind of unification of the chymical with the physical, derived from the universality of forces, whether at the micro- or macro-material level. In this query Newton wrote concerning the *changes* of corporeal things: that "these particles have not only a *vis inertiae*, accompanied with such passive laws of motion as naturally result from that force, but also they are moved by certain active principles, such as is that of gravity, and that which causes fermentation and the cohesion of bodies". The *distinction*, however, is explicit in his alchemical manuscripts; McMullin aptly pointed to the ambiguity of Newton's notion of "active principle", which he sometimes identified with the "laws of nature themselves, made manifest in the phenomena ... [and] are thus fully known, though their 'causes' are not. More often, [Newton] takes the active principle to be itself the cause, the ontological constituent responsible for the motion. The term is thus for him a loose generic one" (McMullin 1978: 81–82).

Huygens too treated the case of the coagulation of milk, assuming that it consisted of a homogeneous mixture of two substances, represented by two types of corpuscles: the milky (serum), which are small and round, and the cheesy, shaped as hooks and eyes. The prolonged application of moderate heat – on this view the occasioning of a moderate degree of motion – increases the intra-corpuscular interactions, resulting in the binding of the cheesy particles and subsequent separation between the two substances. The boiling of milk, on the other hand – that is, imparting an extreme degree of motion – does not allow for such binding. Acrid matters consist of large corpuscles, which impart the same kind of ‘coagulative’ motion to the cheesy particles, as does the slow application of moderate heat (AdS PV, vol. 6: 138r–139r).

Huygens claimed proudly that we could easily imagine such a process and mechanism. For Duclos, this was the crux of the problem, since from a distinctly chymical perspective nothing could actually validate such an explanation, regardless of how imaginable, self-consistent, or ostensibly intelligible it might be. Duclos explained that there are two types of concretions (cohesive processes, causes): congelative and coagulative. The first are occasioned by heat or cold and are interpreted in terms of motion and particles (or sometimes by evaporation); either way, the processes are reversible: cold makes milk congeal into ice and heat reverses the process. In congelative processes there is no essential change in composition. Coagulative processes, on the other hand, are by definition transformative and irreversible since they entail a compositional change, which Boyle might have explained in terms of corpuscular shape, size, and texture. As for coagulations, again, they are of two kinds: the condensative and the transmutational. Condensative coagulation refers mostly to the concretion (cohesion) of particles found within a liquid, as in solutions or as in the curdling of milk; transmutational coagulations are occasioned in processes by which wholesome liquids or liquid-like substances turn into solids, such as sap into wood or food into flesh.

5. Conclusion

Corpuscular motion was for Duclos, as for Huygens or Boyle, a physical phenomenon occasioning changes at the physical level, which is reversible since it involves no essential change. Changes in composition – for Boyle textural and configurational changes – are chymical, essential and irreversible (transmutational). Milk cannot be recovered from cheese, just as the nutrients that went into a plant or animal cannot be regained. Duclos knew this intimately since he supervised and performed large numbers of distillation analyses. Yet again, distillation is occasioned by heat, which according to Duclos, is a superficial means

of decomposition or analysis; it is a physical tool that works only at the level of particles and motion, which renders it analogous (yet directionally opposed) to the process of congelative cohesion.

Returning to Newton's distinction between the mechanical and the vegetal, between the vulgar-chymical and the vegetal-chymical, distillation is indeed associated with vulgar chymistry, in much the same way that a common distiller would have been considered (by Boyle and Duclos alike) as a humble technical chymist who knows how to obtain the various separations by way of controlling the intensity of the fire during distillation. The vegetal is associated with the fermentative (e.g., cheese production), transmutational and vital; it is linked to radical dissolutions and coagulative-generative concretions, all of which belong to the realm of the chymical adept or the philosophical chymist. The latter style of reasoning does not yield itself to structural reductions, neither to denial of action at a distance nor of short intra-corpuscular attraction (affinities, or 'symbole'), an aspect Newton was acutely aware of (see Dobbs 1975, 1982). Just as Roberval and Mariotte criticized Huygens' account of gravity for replacing one mystery with another, Duclos criticized Boyle for proclaiming the rejection of occult substantial qualities by introducing obscure and experimentally unverifiable corpuscular configurations and shapes. Boyle's greatest pride related to his reconciliation of the physical with the chymical, elevating chymistry to a nobler, modern philosophical status, belonging among the ranks of the New Science. For an adept chymist like Duclos this was Boyle's greatest drawback. The two realms are neither metaphysically nor methodologically reconcilable. Boyle's physico-chymical compromise stripped chymistry of its explanatory prowess in both the inanimate and the animate realms. This is why Newton never relinquished alchemy, the categories of which enabled him to account for the vast "variety" of God's creation without reducing it to matter and motion.

Both controversies took place in the Academy, around the same time, and involved some of the same interlocutors. The connections between Duclos' critique of Boyle, the contentious research program on coagulation, and the controversy on the causes of gravity are crucial to understanding the chronological and spatial relations between the two sets of polemical exchanges. The two controversies, however, overlap and interact in several other ways, touching on central themes and processes within the scientific revolution. One of the major achievements of the mechanical philosophy, as advanced by Descartes and later developed by figures like Huygens, was the establishment of new standards of intelligibility. Yet the controversy on the origins of gravity, which largely unfolds on a matter theoretical level, showcases the limitations and circularities entailed by a strictly mechanistic account of attraction, while pointing to the epistemological poverty of such a speculative endeavor. At the same time, as seen in the controversy on coagulation,

the reductive ontology of the mechanical cosmology introduced a new set of problems, most vividly seen in debates on chymistry, broadly construed as the science of both matter and of materials or substances. As seen, the application of the mechanical corpuscular principles to chymistry turned out to be highly problematic. While physically minded chymists like Boyle (and certainly Cartesians like Huygens) sought to merge the 'chymical' with the 'physical', chymists like Duclos viewed the move as reductive and unwarranted. Instead, Duclos, as well as Newton, believed in the need to preserve the autonomy of chymical explanations *alongside* mechanical ones, redrawing the distinctions between the two domains and providing a new ontological and epistemological demarcation.

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Leibniz versus Newton on the nature of gravity and planetary motion

Nir Grannot

1. Introduction

The argument for universal gravity is the most important in Newton’s monumental 1687 *Principia*, but it is also the most controversial. The concept of universal gravity, perceived as action at a distance was widely criticized by eminent natural philosophers such as Christiaan Huygens, Gottfried W. Leibniz and John Bernoulli. Two years after the publication of the *Principia*, Leibniz published his own theory of planetary motion, which tried to imitate the mathematical success of the *Principia* in describing the planetary motion, while rejecting any non-mechanistic explanations for gravity.

Over the next twenty years, the two adversaries maintained a bitter controversy over the nature of gravity and the cause of planetary motion. Although the direct correspondence between the two during that period includes only few short letters, their natural philosophical publications, as well as the correspondence they had with others, reveal an intense and even hostile dispute.¹ The adversaries

1. The following table contains the dates of the texts of the controversy used in this chapter. References for each work appear throughout the paper.

Leibniz	Date	Newton
	1687	<i>Principia</i>
<i>Tentamen de Motuum Coelestium Causis</i>	1689	
Correspondence with Huygenes	1690–94	
	1692–93	Correspondence with Bentley
Letter to Newton	1693	
	1693	Letter to Leibniz
<i>Specimen dynamicum</i>	1695	
	1702	<i>Astronomiae Physicae et Geometricae Elementae</i> (David Gregory)
	1704	<i>Opticks</i>
<i>Antibarbarus Physicus</i>	1706	
<i>Theodicy</i>	1710	

accuse each other of holding a hypothesis “plainly lacking in any foundation ... absurd and wholly unworthy of a philosopher” (Newton 1999: 397), of introducing “fiction invented to support an ill-grounded opinion” (Koyré 1968: 141), of leading us “back to the kingdom of darkness” (AG: 314), and even of atheism (Newton 1999: 398; LE: 677).

The tone of the accusations suggests that this is far from being only a natural philosophical dispute. In fact this controversy exhibits a wide variety of differences between the two interlocutors, theological as well as philosophical. But the core of the controversy is meta-scientific, involving either explicitly or implicitly, questions such as what kind of hypothesis should be admitted as a possible explanation; what is the role (if any) of metaphysical principles in a scientific theory; what are the limits of the inductive method of reasoning; what is the difference between natural (and therefore scientific) and supernatural causes.

In this paper I examine this well known controversy on the nature of gravity and the cause of planetary motion in order to identify its underpinnings. After a short presentation of the contenders’ competing solutions to the problem of planetary motion, I turn to an examination of the different arguments the two adversaries used against each other, as well as of their rhetorical strategies. By this I intend to show that the core of this controversy lies in their different scientific enterprises, which rest on respectively different conceptions of the scope and limits of human knowledge.

2. Two competing theories

The publication of Newton’s *Principia* should naturally be considered as the starting point of the controversy. In proposition VII in book three on the system of the world Newton declares that, “Gravity exists in all bodies universally

(continued)

Leibniz	Date	Newton
Letter to Hartsoeker	1711	
	1712	Letter to Hartsoeker
	1713	Correspondence with Cotes
	1713	2nd ed. of <i>Principia</i>
	1715	Account of the ‘ <i>Commercium Epistolicum</i> ’
Letter to Conti	1715	
	1716	Letter to Conti
Letter to Conti	1716	
Correspondence with Clarke	1715–16	

and is proportional to the quantity of matter in each" (Newton 1999:810). In the previous propositions Newton had first shown that motion according to Kepler's three laws² is identical to motion caused by the existence of a central force, inversely proportional to the square of the distance, and directed to one of the ellipse foci. The mathematical demonstration, part of book one of the *Principia*, made a great impression in Newton's time. It even convinced Huygens that Kepler's laws are more than just approximations of the planetary motions (see Cohen 1987; Harper 2002). The application of this equation to our solar system is contained in propositions I–III in book three. Newton then identified the central force that drives the moon around the earth in particular, and those that put the other celestial bodies in their orbit in general, with the same force that makes bodies on the surface of the earth "heavy" – the force of Gravity (propositions IV–VI in book three). This second step is justified by Newton's two methodological rules, which precede the entire argument, and call for the identification of similar causes to similar effects.

The explanation for the statement in proposition VII is very short, and rests mainly on Newton's third law of motion, according to which every action is always countered by an equal and opposite reaction. Since all the parts that comprise planet A are attracted to planet B, they are, by this law, also attracting planet B, and all the parts that comprise it. In the rest of the *Principia* Newton proceeds to use the existence of the force of gravity to explain a variety of phenomena, such as the sea tides (caused by the attraction of the sun and the moon), the precession of the equinoxes, and especially the movements of comets.

Two years after the publication of the *Principia*, Leibniz published in the *Acta Eruditorum* an essay called *Tentamen de Motuum Coelestium Causis* (essay on the causes of celestial motions), in which he offered his own solution to the problem of planetary motion.³ In the *Tentamen* Leibniz endorses the vortex theory; that is, he accepts that the circular movement of celestial bodies is caused by the circulation of aetherial vortices (see Aiton 1972). In the beginning of the *Tentamen* Leibniz argues in favor of the correctness of the vortex theory by eliminating all other possible solutions:

2. Wilson (2000:225–226) suggests that it was Leibniz in the *Tentamen* who first called Kepler's rules "laws".

3. For an English translation of the *Tentamen*, and a comprehensive background for the work see Meli (1993).

since it seems not at all the province of physics, and indeed unworthy of the admirable workmanship of God, to assign to the stars individual Intelligences directing their course, as if He lacked the means for accomplishing the same by laws governing bodies; and to be sure solid orbs have some while now been rejected, while sympathies, magnetisms and other abstruse qualities of that kind are either not understood, or, when they are, they are judged to be effects consequent on corporeal impressions – I myself judge there is no alternative left but that the cause of celestial motions should originate in the motions of the aether, or, using astronomical terms, in orbs which are deferent, yet fluid. (Meli 1993: 128)

Leibniz's solution to the planetary problem is based on the combination of two movements: a circular movement caused by the aether's circulation, in which the body's velocity that is perpendicular to the radius varies inversely to the radius (Leibniz calls this movement 'harmonic circulation'); and a radial movement along the circulating radius vector ('paracentric movement') which is in turn comprised of two opposing tendencies – the centrifugal tendency away from the center (the outcome of the circular motion), and gravity towards the center.

This combination of movements enabled Leibniz to achieve the required goals: the harmonic circulation is equivalent to Kepler's second law (the area law); the combination of the circular movement and the paracentric movement (which vary with the change in distance due to the varying ratio between the two opposing tendencies) yields the elliptical orbit; and by using his infinitesimal calculus Leibniz calculated the value of the gravity force that would produce the kind of orbit that is followed by planets in our solar system. This value turned out to be identical to the one that Newton had calculated: inversely proportional to the square of the distance (Aiton 1960, 1962, 1964; see also Westfall 1971: 303–310).

Newton's name is mentioned only once in the *Tentamen*. According to Leibniz's testimony, he did not have the opportunity to read the *Principia* prior to the publication of the *Tentamen*. He did, however, come across a review of the *Principia* in the *Acta Eruditorum*, which stimulated him to publish notes he made twelve years earlier (Meli 1993: 8; Cohen 1992: 323–338). While this testimony was argued to be false in recent years (Meli 1993: 9; Antognazza 2009: 296), the originality of his work remained beyond doubt. It is obvious that the publication of the *Tentamen* was meant to serve as an alternative to the *Principia*, and as such it had to meet a condition – to account for the same phenomena that the *Principia* accounted for. In the *Tentamen* Leibniz explained the motion of a planet around the sun. But in the concluding paragraph he admits that two major issues remained unresolved: the first is the cause of gravity (more precisely, what motion of the aether causes the gravitational tendency of the planets towards the sun), the second is to explain why the planets move in accordance with Kepler's third law (the periodic times are in the sesquialterate proportion of the mean

distances). The cause of gravity was discussed in a second, unpublished version of the *Tentamen* (GM, 6: 161–187),⁴ in which Leibniz followed Huygens' explanation of spherical vortices circulating in all directions. The circulation of the vortices produced the required centrifugal force of dense matter away from the center, which in turn pushed bigger and more porous particles towards the center.

Leibniz's first reaction to the *Principia* is found in his letters to Huygens. Like Huygens, Leibniz did not fail to appreciate Newton's achievement. And so, even when he praised his own theory, he claimed that "harmonic circulation alone has the property that the body circulating in this way keeps exactly the force of its direction or previous impression as if it moved in a vacuum merely by its own impetuosity together with [*jointe à*] gravity" (AG: 310), thus implicitly acknowledging the simplicity and accuracy of Newton's theory. But there was a certain aspect of this theory that he could not approve of. He wrote to Huygens:

I do not understand how he [Newton] conceives gravity, that is, attraction. It seems that, according to him, it is only a certain incorporeal and inexplicable power [*vertu*], whereas you explained it very plausibly by the laws of mechanics.
(AG: 309)

For Leibniz, like many other scientists of his age, a scientific explanation had to remain within the mechanistic framework (i.e. everything in nature is explained through size, shape and motion of matter, and nothing moves naturally except through contact and impact). Any other explanation seemed like a reversion to the scholastic appeal to occult qualities. But more is involved here:

for although Newton is satisfactory when one considers only a single planet or satellite, nevertheless, he cannot account for why all the planets of the same system move over approximately the same path, and why they move in the same direction, using only impetuosity [*trajection*] together with gravity. That is what we observe, not only for the sun's planets, but also for those of Jupiter and those of Saturn. This is good evidence for there being a common reason that determines them to behave in this way; and what other more probable reason can be brought to bear than that some kind of vortex or common matter carries them around? *For to have recourse to the decision of the author of nature is not sufficiently philosophical when there is a way of assigning proximate causes.*

(GM, 6: 189–193; AG: 309–310; italics added)

According to Leibniz, Newton's theory lacks explanatory power: it does not give a proper (i.e., mechanical) explanation of the force of gravity, and even an important phenomenon such as the movement of the planets on the same plane and in

4. For a commentary of the differences between the versions see Meli (1993: 155–161) and Koyré (1968: 133–137).

the same direction is left unexplained. Leibniz's theory of vortices, on the other hand, explains all this neatly, within the accepted mechanical framework (Leibniz used vortices not only to explain the planetary motion, but also to explain the notion of solidity (*Specimen Dynamicum*, GM, 6: 234–254; AG: 135–136).

Newton had in fact already rejected the vortex theory in the *Principia*, mostly due to its incompatibility with the movement of comets, as he wrote to Richard Bentley:

the motions which the planets now have could not spring from any natural cause alone, but were impressed by an intelligent agent. For since comets descend into the region of our planets, and here move all manner of ways, going some times the same way with the planets, sometimes the contrary way, and sometimes in cross ways, in planes inclined to the plane of the ecliptic, and at all kinds of angles, it is plain that there is no natural cause which could determine all the planets, both primary and secondary, to move the same way and in the same plane, without any considerable variation. *This must have been the effect of counsel.*

(NC, 3:234–235; italics added)

In his 1693 letter to Leibniz – the only letter Newton sent him during this controversy – he responded to Leibniz's claim that the planetary motion was “caused or regulated by the motion of a fluid medium” (NC, 3:258), by saying that

since all phenomena of the heavens and of the sea follow precisely, so far as I am aware, from nothing but gravity acting in accordance with the laws described by me; and since *nature is very simple*, I have myself concluded that *all other causes are to be rejected* and that the heavens are to be stripped as far as may be of all matter, lest the motions of planets and comets be hindered or rendered irregular.

(NC, 3:287; italics added)

It seems that in the choice between an accurate and simple description of the phenomena, and a reasonable explanation, Newton favors the former. “But” he continues “if, meanwhile, someone explains gravity along with all its laws by the action of some subtle matter, and shows that the motion of planets and comets will not be disturbed by this matter, I shall be far from objecting” (*ibid.*). Newton shifts the burden of proof to Leibniz's side, and thus poses a “controversy's demand” (Dascal 1990: 72): the vortices theory is rejected, and it is now Leibniz's duty to either find another theory that will account for all the phenomena of the heavens or else to accept Newton's theory.

So far, the controversy revolved mainly around physical and metaphysical arguments. Leibniz's theory, to be sure, contained numerous unresolved difficulties and inconsistencies. The problem of the movements of comets, and also the inconsistency of harmonic circulation with Kepler's third law, which were pointed out by David Gregory in 1702, remained without a convincing answer, leading to

the conclusion that “Leibniz could not do what could not be done, namely reconcile Kepler’s laws with the circulation of a vortex” (Aiton 1972: 136–137.) It is no wonder, therefore, that Leibniz did not pick up the glove, although he did not neglect the vortices theory, and in 1706 published another version of the *Tentamen* with corrections, and some amendments; instead he shifted the debate to more explicit theological, metaphysical and methodological grounds.

3. Natural versus miraculous

As we have seen, in his letters to Bentley Newton turned to the will of God in trying to explain a variety of physical phenomena (among others are the difference between lucid stars and opaque planets, and the fact that only one body in our solar system is able to give light and heat to all the rest).⁵ This recourse to the will of God, in explaining natural phenomena, which can also be found in the queries of his 1706 *Opticks*, continued to form the target of Leibniz’s criticism. In a letter he sent to Nicholas Hartsoecker in 1711, which was later published in the *Memoirs of Literature*, he introduced the ironical notion of a “perpetual miracle.” Leibniz wrote:

For example, if any one should say, it is God’s will that a planet should move round in its orbit, without any other cause of its motion, I maintain, that it would be a perpetual miracle: for by the nature of things, the planet going round tends to remove from its orbit through the tangent, if nothing hinders it; and God must continually prevent it, if no natural cause does it. (Koyré 1968: 141)

The notion of “perpetual miracle” was used by Leibniz to criticize views that introduced unnecessary divine intervention into the universe, such as those of Malebranche and the occasionalists (Vailati 1995: 570). This kind of continuous intervention, he claimed, would diminish the greatness of God and the perfection of his creation. Leibniz used two related definitions of the notion of ‘miracle’: that which exceeds the power (nature) of created beings, and that which is inexplicable to the human mind (McRae 1994: 392).

A natural cause, then, is one that is carried out by and in accordance with the nature of things. A supernatural or miraculous one is that which contradicts the nature of things, and hence requires God’s intervention. Here an objection might be raised: since God’s will is the ultimate cause of all things, this distinction between natural and supernatural causes is a superfluous one:

5. On Newton’s willingness to introduce explanations which included God’s intervention in the world, see Freudenthal (1986:44–70). Freudenthal also gives an interesting account of the use of the clock as a scientific model in this controversy.

It may be said in a very good sense that every thing is continued miracle, that is, worthy of admiration: but it seems to me that the example of a planet which goes round and preserves its motion in its orbit without any other help but that of God, being compared with a planet kept in its orbit by the matter which constantly derives it towards the sun, plainly shows what difference there is between reasonable natural miracles, and those that are properly so called, or supernatural; or rather *between a reasonable explication, and a fiction invented to support an ill-grounded opinion*. Such is the method of those who say, after Mr. De Roberval's Aristarchus, that all bodies attract one another by a law of nature, which God made in the beginning of things. For alleging nothing else to obtain such an effect, and admitting nothing that was made by God whereby it may appear how he attains to that end, they have recourse to a miracle, that is, to a supernatural thing, which continues forever, *when the question is to find out a natural cause*.

(Koyré 1968: 141; italics added)

What distinguishes a natural cause from a supernatural one is the existence of some kind of mechanism that fulfills the laws of nature. This mechanism serves as a cause, and at the same time is also intelligible and explicable. The supernatural cause, on the other hand, is inexplicable and arbitrary. The purpose of a scientific theory is to "find out a natural cause", i.e. to find an explanation to the natural phenomena, and Newton, so Leibniz claims, fails to do exactly that. It is important to note, however, that Leibniz is not criticizing Newton for not finding the cause of gravity. It is rather the type of explanation Newton is using which is being attacked:

Thus the ancients and the moderns, who own that gravity is an occult quality, are in the right, if they mean by it that there is a certain mechanism unknown to them, whereby all bodies tend towards the center of the earth. But if they mean that the thing is performed without any mechanism by a simple primitive quality, or by a law of God, who produces that effect without using any intelligible means, it is an unreasonable occult quality, and so very occult, that *it is impossible it should ever be clear, though an angel, or God himself, should undertake to explain it*.

(Koyré 1968: 141; italics added)

According to Leibniz, then, an occult quality is accepted when it is used as a tentative replacement of an unknown mechanism, not when it serves as a substitution for such a mechanism. Newton is not accused of rejecting (or not committing himself to) a certain mechanism, but of rejecting (or not committing himself to) the need for any mechanism whatsoever. Again, Leibniz stresses the importance of such a mechanism, not just as a natural cause but also as a reasonable explanation. The demand is that the cause must be intelligible and explicable – the laws of nature must obey the laws of reason.

Newton's reaction is found in an unpublished letter, intended for the editor of the *Memoirs of Literature*. In this letter he quotes Leibniz's accusations, and answers them one by one. First of all, he says, it has been proved that all the bodies on the surface of the earth gravitate to the earth in proportion to the quantity of matter in each of them, and that by the same law the moon tends to the earth and the planets tend to each other; and that this tendency is responsible for all their motions. "These things have been proved by mathematical demonstrations grounded upon experiments and the phenomena of nature: and Mr. Leibniz himself cannot deny that they have been proved". His only sin, he protests, is that he failed to commit himself to the mechanical hypothesis:

Because they do not explain gravity by a mechanical hypothesis, he [Leibniz] charges them with making it a supernatural thing, a miracle and a fiction invented to support an ill-grounded opinion and compares their method of philosophy to that of Mr. De Roberval's Aristarchus, which is all one as to call it romantic. They show that there is a universal gravity and that all the phenomena of the heavens are the effect of it and with the cause of gravity they meddle not but leave it to be found out by them that can explain it, whether mechanically or otherwise. And doth it deserve to be scouted with the language of a supernatural thing, a miracle, a fiction invented to support an ill-grounded opinion, and a method of philosophy after Mr. Roberval's romance?

(NC 4: 299)

Newton rejects Leibniz's criticism as unjustified: Newton had proved that universal gravity is responsible for the planetary motions using mathematical demonstrations and experiments, something that even Leibniz cannot deny (not quite so, as we shall see), and as to the cause of gravity, he has not found one (yet), and therefore refrained from committing himself to any specific explanation.

Let us take a closer look at Newton's defense line against Leibniz's accusations. What is the difference between Newton's force of gravitation and the occult qualities of the scholastics? Is it not the case that both instances comprise inexplicable 'black boxes', invoked to explain a certain phenomenon? In his 1706 *Opticks*, in the 23rd query (which later became query 31) Newton tries to clarify the difference:

These Principles I consider, not as occult Qualities, supposed to result from a specifick Forms of Things, but as general Laws of Nature, by which the Things themselves are form'd; their Truth appearing to us by Phenomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult. And the Aristotelians gave the Name of occult Qualities, not to manifest Qualities, but to such Qualities only as they supposed to lie hid in Bodies, and to be the unknown Causes of manifest Effects: Such as would be the

Causes of Gravity, and of magnetick and electrick Attractions, and of Fermentations, if we should suppose that these Forces or Actions arose from Qualities unknown to us, and incapable of being discovered and made manifest. Such occult Qualities put stop to the Improvement of natural Philosophy, and therefore of late Years have been rejected. To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phenomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above-mention'd, they being of very general Extent, and leave their Causes to be found out.

(Newton 1952:401–402)

Several things mark the difference between Newton's force and a scholastic occult quality. First, Newton draws a line between 'manifest' and 'occult' – while the causes are occult, the qualities are manifest. This might raise the following question, which was actually raised by Bernard de Fontenelle (Aiton 1972:109): were not the scholastic occult qualities also causes of manifest effects? If the effect is occult, isn't it precisely so because of the lack of proper explanation? The whole mystery lies in the cause of a certain phenomenon. It seems that Newton is referring to the difference between qualities belonging to the bodies themselves, and laws of nature, dictating (or describing) the bodies' behavior. The scholastic occult qualities are intrinsic to the body, part of its essence. When assigning such a quality to a body, one is not clarifying any mystery, nor explaining what needs to be explained. Instead, a new quality is invoked, one that is hidden and inaccessible. Laws of nature, on the other hand, are extrinsic to the bodies. When using a law to describe the natural phenomena, Newton is not invoking any entity or quality. He is simply describing the bodies' behavior, which in itself can be demonstrated. Newton, it must be admitted, maintained all along that gravity is not inherent to matter:

That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.

(Newton to Bentley; NC, 3:254)

The fact remains, though, that this position was often mistakenly ascribed to him by adversaries and advocates alike, mainly because of his reluctance to mention any medium that carried out this action (Koyré 1968: 154). Leibniz, too, sometimes ascribed to Newton this position (Leibniz 1990: 85–86), but this does not necessarily reflect a misunderstanding on his behalf. Leibniz rejects the view that laws of nature can exist in a disembodied way, separated from the things they describe (Ishiguro 1994: 408). Laws do not “bestow upon things only an extrinsic denomination” but rather an “internal law from which their actions and passions follow” (LE: 500). This internal law is nothing but their nature. The universe operates in a regular way according to laws, but ultimately the source of action in the world lies in the things themselves. For gravity to be counted as a law of nature, it must be, in some way or another, imprinted onto the things themselves.

The second thing that marks the difference between a scholastic occult quality and Newton’s law of gravity is the latter’s generality. Instead of invoking a specific quality for every species of things, the force of gravity explains the behavior of all corporeal things. This generality goes hand in hand with the metaphysical assumption of the simplicity and uniformity of nature.

But the most significant difference between Newton’s force of gravitation and a scholastic occult quality, without which this generality is useless, is the powerful mathematical quantification and accuracy which were applied to describe a variety of phenomena. It should be noted that the idea of universal gravitation was not a new idea. Among others, it was held, as Leibniz notes in his accusations, by Gilles Personne de Roberval some fifty years before the publication of the *Principia* (Aiton 1972: 90; Koyré 1968: 59, note 2). But until that time, no one gave this idea, or, indeed, any other scientific idea, the kind of mathematical outfit that was advanced by Newton, turning it into a model for a mathematical demonstrative science. The unification of so many phenomena, terrestrial as well as celestial, ranging from the smallest particles to the biggest heavenly bodies, under a single mathematical equation, was indeed very compelling, and must have given the impression that the existence of this universal force is uncontested. Yet this does not make the concept of universal gravity more intelligible. Leibniz, as we have seen, had no objection to the mathematical description, nor to the manifest effects of gravity. What he refused to accept was the lack of any mechanism made by God to achieve that effect, and so Newton’s defense could not meet his objection, that one cannot reject the mechanical explanation while holding the tentative excuse.

But Newton had more to say about the demand for a mechanical explanation:

So then gravity and hardness must go for unreasonable occult qualities unless they can be explained mechanically. And why may not the same be said of the vis inertiae and the extension, the duration and mobility of bodies, and yet no man ever attempted to explain these qualities mechanically, or took them for miracles or supernatural things or fictions or occult qualities ... Leibniz himself will scarce say that thinking is mechanical as it must be if to explain it otherwise would be to make a miracle, an occult quality, and a fiction. (NC, 4: 300)

The analogy to other essential attributes of matter and even to the process of thinking is clear; the demand for a mechanical explanation for everything is something that even Leibniz himself cannot fulfill. There must be, therefore, other natural explanations beside the mechanical one, explanations that should not be labeled as occult or miraculous. By breaking the identification of 'natural cause' with 'mechanistic cause', as held by Leibniz, Newton can reject the mechanical explanation yet still argue that he is not rejecting the need for a natural cause for gravity.

Leibniz, as it turns out, agrees with Newton that mechanical explanations are insufficient, but for different reasons. In *Anti Barbarus Physicus*, an essay devoted to an attack on Newton's new philosophy, Leibniz distinguishes himself from the other mechanistic philosophers (Galileo, Descartes, Hobbes) which "have not sufficiently recognized the true metaphysical principles or the explanations of motion and laws of nature that derive from them". "Therefore", he continues

I tried to fill this gap, and have at last shown *that everything happens mechanically in nature, but that the principles of mechanism are metaphysical*, and that the laws of motion and nature have been established, not with absolute necessity, but from the will of a wise cause, not from a pure exercise of will, but from the fitness [*convenientia*] of things. (AG: 319; italics added)

He then proceeds to elaborate his own metaphysical stance, claiming that

everything in bodies takes place through shape and motion, everything in souls through perception and appetite ... and that souls and bodies, though infallibly following their own laws, agree nevertheless through a harmony pre-established by God, without any physical influx between one another. (Ibid.)

The laws of nature, says Leibniz, are neither necessary nor arbitrary. They are established "from the fitness of things", that is, by reason (ibid.). Leibniz's central principle in the sphere of natural phenomena is the principle of sufficient reason. According to this principle nothing in nature happens without a reason for it to happen. In order to understand nature, therefore, we have to look for the reasons. These reasons can be either physical or metaphysical; both types of explanations are in harmony with each other (Garber 1994: 327). When we look for the final

causes of the natural phenomena we ultimately have to resort to the wisdom of God. But while this kind of explanation is always true, it is not sufficient in natural philosophy, where we look for the realization of God's wisdom, or the efficient causes. This realization is carried out by mechanical means (Garber 1994:328). The demand for a mechanistic explanation is, therefore, a demand for a reason: "in physics everything happens mechanically, that is, intelligibly," and so,

those who have shown that the astronomical laws can be explained by assuming the mutual gravitation of the planets have done something very worthwhile, even if they may not have given the reason for this gravitation. But if certain people, abusing this beautiful discovery, think that the explanation [ratio] given is so satisfactory that there is nothing left to explain, and if they think that gravity is a thing essential to matter, then they slip back into barbarism in physics and into the occult qualities of the Scholastics. (AG:314)

This ultimate turn to metaphysics and Leibniz's elaborated metaphysical system are of course rejected by Newton, who turns Leibniz's accusations on their head: "His pre-established harmony is miraculous, and contradicts the daily experience of all mankind, every man finding in himself a power of seeing with his eyes, and moving his body by his will" (NC, 6:285–286; Alexander 1956:187).

We can see that Newton and Leibniz disagree about what is natural, and what is miraculous, a disagreement that was later echoed in the correspondence between Leibniz and Samuel Clarke. Newton and Clarke share the view that a miracle is something that happens very rarely, and that "unusualness is necessarily included in the notion of a miracle" (Clarke's 4th reply; LE:695). Something that occurs regularly, therefore, cannot be miraculous: "the moyen by which two bodies attract each other may be invisible and intangible, and of a different nature from mechanism, and yet, acting regularly and constantly, may well be called natural" (ibid.:696).

For Leibniz a miracle is "any event which can only occur through the power of the Creator, its reason not lying in the nature of created things" (NC 6:307), which has nothing to do with the frequency by which it occurs. In his 5th reply to Clarke he claims that:

If a miracle differs from what is natural only in appearance and with respect to us, so that we call that only a miracle which we seldom see, there will be no internal real difference between a miracle and what is natural, and at the bottom everything will be either equally natural or equally miraculous.

(Leibniz's 5th reply; LE:715)

This is why the mutual attraction between bodies cannot be counted as anything but miraculous:

That means of communication, says he, is invisible, intangible, not mechanical. He might as well have added inexplicable, unintelligible, precarious, groundless, and unexampled. But it is regular, says the author; it is constant and consequently natural. I answer, it cannot be regular without being reasonable, nor natural unless it can be explained by the natures of creatures.

(Leibniz's 5th reply; LE:716)

Newton's and Clarke's demarcation between the natural and the miraculous lies at the level of appearances – the frequency by which a certain phenomenon occurs. Science is concerned with this level alone, for we have no access to any inner nature of bodies. Leibniz, in contrast, seeks to find the difference at the metaphysical level assuming that there must be something in the constitution of things, in their internal powers, which makes them behave the way they do. Exceeding these natural powers, even on a regular basis, is unnatural, or miraculous. The demarcation between the natural and the miraculous is, therefore, also the demarcation between the reasonable and the unreasonable. Underlying these theological and metaphysical differences is an epistemological disagreement over the questions whether the internal structure of nature is accessible to human understanding or not.

4. Methodology and the role of hypotheses

In 1713 Newton wrote some instructions and clarifications to Roger Cotes, the editor of the upcoming edition of the *Principia*. The main subject of these letters concerned Newton's scientific methodology in relation to his rivals. In a draft of an unsent letter to Cotes, he writes:

Experimental philosophy reduces phenomena to general rules and looks upon the rules to be general when they hold generally in phenomena. It is not enough to object that a contrary phenomenon may happen but to make a legitimate objection, a contrary phenomenon must be actually produced. Hypothetical philosophy consists in imaginary explications of things and imaginary arguments for or against such explications, or against the arguments of experimental philosophers founded upon induction. The first sort of philosophy is followed by me, the latter too much by Descartes, Leibniz, and some others. (NC, 5: 398–399)

We are now in a better position to understand Newton's refusal to commit to a certain explanation of the cause of gravity. The distinction Newton draws between the existence of gravity and the cause of it is based on the distinction between two different scientific methods. The existence of universal gravity and the derivation of all heavenly motion from it were "proved by mathematical demonstrations

grounded upon experiments and the phenomena of nature". The cause of gravity, on the other hand, for lack of decisive experiments, lies beyond the scope of experimental philosophy. The explanations given to it so far (the mechanical hypothesis) are "not a phenomenon nor deduced from any phenomena but assumed or supposed without any experimental proof" (NC, 5:398), and therefore, lay in the region of "imaginary explications of things" (NC, 5:397). The experimental/hypothetical distinction, widespread in the second half of the seventeenth century (Anstey 2005:215), is used to exclude certain kind of explanations and arguments from natural philosophy. In this account, the legitimacy of claims in natural philosophy stems from their connection to phenomena. It is only by observations and experiments that we can explore the physical universe. Imaginations and speculations, clearly unfavorable notions in this context, are regarded as unreliable tools for this task.

Leibniz's metaphysics, as well as the arguments he advances against Newton are, therefore, deemed "imaginary". Leibniz, as we have previously seen, argued that the laws of motion and other laws of nature are derived from metaphysical principles. Newton, in contrast, claims that they were established by the inductive method:

In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method. And it is enough that gravity really exists and acts according to the laws that we have set forth and is sufficient to explain all the motions of the heavenly bodies and of our sea. (Newton 1999:943)

For Newton, a fundamental principle of the scientific method lies in pushing the empirical generalizations as far as we can, and seeking general laws that will account for as many phenomena as possible. These generalizations should not be constrained by any hypothetical or speculative considerations (Stein 1970:263). The general laws should then hold as long as no counter phenomenon can be found. Newton is well aware of the limits of the inductive inference, and admits that "the generality cannot be demonstrated a priori" (NC, 5:399). It is not surprising then that whenever he defends his theory against criticism he does so by pointing out its extensive agreement with observed phenomena, and not the validity of the argument that led to it. It is the probative strength of the theory which matters, and which enables him to declare that "gravity really exists". The declaration simply stands for the acceptance of an inductive inference.

Yet it is exactly this kind of inference from particular experiments and observations to general propositions and laws which was criticized by Leibniz in the *Anti Barbarus Physicus*, where he warned:

we must be careful not to proceed from a few instances to everything, as Gilbert sees a magnet everywhere, and the chemists smell salt, sulphur, and mercury everywhere. Such explanations are usually considered insufficient, and sometimes we derive not only things of uncertain existence from assuming such explanations, but also false and impossible things, like that general striving [*nisus*] of matter for matter. (AG: 314)

The problem with induction, pointed out by Leibniz on many occasions, is not only that its results are uncertain but that it can be sometimes positively misleading. Leibniz does not reject induction altogether but insists that an explanation should not rest solely on this kind of inference; since it is insufficient, it should be supported by reasoning.

The distinction between the two scientific methods is stressed even further in Newton's 1715 anonymous "An account of the Book Entitled *Commercium Epistolicum*" where he notes:

It must be allowed that these two gentlemen [(Newton and Leibniz)] differ very much in philosophy. The one proceeds upon the evidence arising from experiments and phenomena, and stops where such evidence is wanting; the other is taken up with hypotheses, and propounds them, not to be examined by experiments, but to be believed without examination. (Quoted in Koyré 1968: 147)

More than just a methodological distinction, the experimental/hypothetical distinction is also an epistemic one: where there is no evidence there can be no knowledge. While Newton himself is very careful not to cross this epistemological line, Leibniz seems to drift beyond it into the realm of the imaginary.

In 1715 Leibniz wrote a letter to the Abbé Conti, in which he responded to the distinction that Newton drew between his own 'experimental philosophy' and Leibniz's 'hypothetical philosophy'. While accepting the experimental method, and even referring to the great Lord Verulam to stress his commitment to it, he defends the right of holding provisional hypotheses:

I strongly approve of his method of drawing from phenomena what can be drawn without making any suppositions, even if sometimes this is only drawing conjectural consequences. However, when the data are not sufficient, it is permissible (as one does sometimes in deciphering) to imagine hypotheses, and if they are good ones to hold them provisionally, waiting for new experiments to bring us nova data and for what Bacon calls *experimenta crucis*, in order to choose between hypotheses. (Quoted in Alexander 1956: 186)

In fact, Leibniz continues, Newton himself is doing just that:

I am strongly in favour of experimental philosophy, but M. Newton is departing very far from it when he claims that all matter is heavy (or that every part of matter attracts every other part) which is certainly not proved by experiment ... And because we do not yet know in detail how gravity is produced, or elastic force, or magnetic force, this does not give us any right to make of them scholastic occult qualities or miracles; but it gives us still less right to put bounds to the wisdom or power of God. (Ibid.)

This is an important accusation, for Newton seemingly pretends to “content himself with certainties and let uncertainties alone” (quoted in Koyré 1968: 146); but this is clearly not the case. Leibniz makes it very clear that empirical data is the basis of natural philosophy. But Newton’s universal gravity is far from being supported by such empirical evidence. It is nothing but a conjecture, a hypothesis. And while hypotheses are required, at least provisionally, they must remain within the accepted explanatory (i.e., mechanical) domain.

Newton and Leibniz are not arguing over the empirical data, but rather about the criteria for admitting hypotheses in the absence of such data. Newton’s use of the term ‘hypothesis’ had several meanings, and it evolved during the controversy. It had a “good” meaning – that of a plausible conjecture not yet proven, and a “bad” meaning – that of a fictitious and even false belief (Koyré 1968: 36–37). The vortices theory, the hypothesis Leibniz offered as a reasonable conjecture belonged to the second kind: hypotheses which are introduced as premises or explanations with no empirical foundation. It is this kind of hypotheses that Newton had in mind when he famously declared “hypotheses non fingo”. Newton assigned much more importance to the probative power as a criterion for choosing between hypotheses,⁶ while Leibniz insisted on the need for intelligibility.

These two methodological views on the role of hypotheses are based on different metaphysical assumptions: Newton’s demand, that we push induction as far as possible, is based on the assumption that nature is essentially simple and uniform. Leibniz, while not rejecting this assumption, assumes (or, better, demands) that nature be rational. Any hypothesis therefore must be rational, that is, intelligible. These two metaphysical choices did not come without a price: Leibniz

6. Stein argues that Newton’s argument for universal gravity in the *Principia* cannot be properly called deduction from the phenomena. Indeed, it was rather the “new (and confirmed) astronomical discoveries, and a great increase in both the scope and the precision of astronomical prediction, provide a kind of warrant for that law that can quite reasonably be seen as drawing the sting from the charge of ‘wild hypothesis’ that could otherwise be leveled at Newton’s way of applying the third law of motion” (Stein 1990: 220).

encountered great theoretical and mathematical difficulties and inconsistencies for holding the mechanical explanation, while Newton had to give up intelligibility for his simple and unifying mathematical solution (throughout his career Newton never gave up the search for a reasonable explanation for the force of gravity (Cohen 1987: 587–588).

Finally, beside the methodological disagreement there is also a procedural one, for both natural philosophers differ on the procedures they establish in order to settle the controversy. While Newton claims that “it is not enough to object that a contrary phenomenon may happen but to make a legitimate objection, a contrary phenomenon must be actually produced” (NC, 5: 398), laying the burden of proof (in this case, the burden of refuting his theory by finding the appropriate evidence) on Leibniz, the latter holds that it is Newton’s duty to prove by experiment his “ill supported” theory, or else abandon it.⁷

5. Conclusion

With the death of Leibniz in 1716 the actual controversy ended, but it remained unresolved. Although the adversaries used different arguments and argumentative strategies throughout the controversy, there was no change of mind on either part. Both held throughout the debate onto their original views, refusing even to approach the other’s position. Turning back to their initial publications in the controversy we can notice that they are engaged in two different scientific programs. In the *Principia*, Newton claims that the main task of philosophy is “to discover the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces” (Newton 1999: 382). The natural philosopher’s task, therefore, is to give a *mathematical description* of the laws of motion. Leibniz, on the other hand, intends in his *Tentamen* to “explain more distinctly the laws of motion themselves” (Meli 1993: 128), and to “come close to *the true causes* of celestial motions” (ibid.; italics added), describing the task of the natural philosopher as one of explaining rather than describing.

7. In Dascal’s terminology, this procedural disagreement turns the controversy into a ‘dispute’ – a polemical exchange in which, among other characteristics, “there are no mutually accepted procedures for deciding the dispute, that is, a dispute has no solution; at most it can dissolve or be dissolved”. See Dascal (1998: 20).

Leibniz's demand for an explanation throughout the controversy reflects his belief that the laws of nature are subordinated to the laws of reason:

It seems that we must concede that God always acts wisely, that is, in such a way that anyone who knew his reasons would know and worship his supreme justice, goodness, and wisdom ... since we cannot know the true formal reason for existence in any particular case because it involves a progression to infinity, it is therefore sufficient for us to know the truth of contingent things a posteriori, that is, through experience, and yet, at the same time, to hold, universally or in general, that principle divinely implemented in our mind, confirmed both by reason and experience itself (to the extent that we can penetrate things), that nothing happens without reason ... And just as God himself decreed that he would always act only in accordance with true reasons of wisdom, so too he created rational creatures in such a way that they act only in accordance with prevailing or inclining reasons, reasons that are true or, in their place, apparent. (AG: 29)

God's will is not arbitrary, but rational, and hence explicable (this is why Leibniz refuses to accept things that we cannot understand even if "God himself should undertake to explain it"). Leibniz identifies rationality with intelligibility, for it is the same rationality that governs the divine creation of the universe that is also implemented in our minds. There is no boundary that separates nature from reason; there is nothing in nature that is not explicable; the human ability to understand nature is not limited. This does not mean, however, that complete understanding can ever be achieved: "But in contingent propositions one continue the analysis to infinity through reasons for reasons, so that one never has a complete demonstration, though there is always, underneath, a reason for the truth, but the reason is understood completely only by God, who alone traverses the infinite series in one stroke of mind" (AG: 28). Much like a mathematical limit, the workings of nature can be approximated, but never fully grasped. On Leibniz's view, Newton's philosophy turns nature into an arbitrary system and thus limits the rationality of God's creation.⁸

Newton's concept of rationality is much more modest. Our scientific inquiry rests upon arguments and reasons, but we cannot hope to truly understand the secrets of the universe:

8. Koyré puts it nicely when he writes that "...Newtonian cosmology, which, with respect to those that preceded it, represents a unification and an admirable simplification of the laws that regulate the universe, does not diminish, but on the contrary increases, at least apparently, the accidental and irrational character of the planetary system" (Koyré 1968: 203).

To myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.

(Quoted in Cassirer 1943: 380)⁹

For Newton, the only sources of natural knowledge are the phenomena themselves. By observing nature and by experimenting we can draw general propositions that will hold true as long as no counter phenomenon will be found. Leibniz's argumentation is hence perceived as invalid: for a theory to be rejected it should be rejected by nature itself. This leaves many things outside the scope of human knowledge, perhaps not just temporarily but in principle. We are, however, left with knowledge instead of imaginary explications of things.

We can finally conclude that underlying the natural philosophical, metaphysical, and theological differences and disagreements in this controversy there is an epistemological gulf. On the one side stands Leibniz, who can accept no boundary to the scope and reach of human understanding, nothing that separates human rationality from divine rationality. On the other stands Newton, who believes that the only source of our knowledge of nature is nature itself, which is "accessible to, but not penetrable by, human reason" (Cassirer 1943: 380).

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[Abbreviations indicated at the end of each entry.]

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9. Cassirer quotes this famous self observation, and then concludes: "Science may lead us very far, but it cannot hope to probe into the real depth of the "ocean of truth". This depth remains immeasurable and unfathomable to human thought".

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The argumentative use of methodology

Lessons from a controversy following Newton's first optical paper

Gábor Á. Zemlén

1. Scientific debates and the emergence of modern science

Controversies arose again and again during the emergence of modern science. The historian¹ sees the actors questioning and challenging each other's contributions in all areas and levels of inquiry. The style varies from polite correspondence to aggressive *ad hominem* attack, from inquisitive query to *ad baculum* retort; the medium ranges from private letter to pamphlet, book, or journal article, and the topics stretch from questioning data through criticizing methodology, challenging priority or even intellectual honesty.

Not only countless controversies, but also numerous changes characterize the period; changes in the methods propagated and used, in the manner of disseminating results, in the running of institutions. In short, changes in the way science is carried out. Did the controversies during this period play any (constitutive) role in these changes? If so, in which areas, to what extent, and how?

As a single case is sufficient to illustrate many of the problematic issues that, unless resolved, stand in the way of answering these questions, I focus on a particular controversy and on the historiographical tradition of its reconstruction. The controversy in question, one of the first major debates of the Scientific Revolution to occur in a journal, erupted after the publication of Isaac Newton's first paper in the *Philosophical Transactions*, which outlined his theory of white light and colors (Newton 1671–72).² After a short overview of the debate, I investigate

1. In argumentation theory 'analyst' would be more appropriate (see also the actor-analyst distinction), as, however, the paper mainly discusses matters of interpretation in history of science, 'historian' will be used throughout the paper.

2. This is the first significant and extensive controversy to occur in the *Philosophical Transactions*, though, of course, 'scientific significance' is understood retrospectively.

a series of problems concerning the reconstruction of the position and the use of methodology in argumentative situations. Addressing problems of reconstruction will help us determine the role of controversies in shaping the ideas and ideology of modern science. I argue for the viability of the ‘radical dialectification’ of controversies, a process of reconstruction that is charitable yet is highly sensitive to certain types of inconsistencies in positions.³

2. A brief overview of the controversy and its historiography

When asked by the Royal Society to describe his invention – a novel type of telescope – Newton decided instead to share with the Society his “New Theory about Light and Colours” (Newton 1671–72). The article published in the *Philosophical Transactions* of the Royal Society was short, and was partly based on Baconian ideals. Newton used the recent notion of crucial experiments to *prove* his discovery of the different refrangibility of various light rays.⁴ The paper also contained an elimination of rival explanations before arriving to the *experimentum crucis* (Raftopoulos 1999), and to Newton’s theory of colors in the form of propositions.

The controversy took place among well-known figures of the republic of letters, first Robert Hooke, and later Christiaan Huygens, but also less illustrious correspondents, including Jesuit scholars such as Gaston Ignace Pardies and Anthony Lucas. It had all the elements of controversies as described above, ranging from the polite to the aggressively *ad hominem*, touching on epistemic, methodological, and moral issues. Although the episode has been studied extensively in the past, disagreements among historians remain significant.

With a few exceptions, Newton’s supremacy in the debate was generally unquestioned by early historians (Rosenfeld 1927; Whewell 1837).⁵ On this account,

3. These issues require both the investigation of the original sources and of the various historiographical traditions that have assumed different positions with regard to these texts. An informed analysis and criticism of the primary sources as well as of the historiographical approaches, however, cannot be carried out in detail here. To comply with both desiderata, rather than being descriptive and systematic with respect to either specific controversies or the historiography, I aim to use as well as thematise specific *issues* pertaining to the reconstruction. These issues arise when approaches and methods generally used for studies of controversies are contrasted, explicated, examined, and problematised.

4. The term *experimentum crucis* was borrowed from Robert Hooke, who conflated two expressions of Bacon.

5. The most notable exception is Goethe’s *Farbenlehre* from 1810, also giving an early account of the controversy in the historical part of the book.

the positions of Newton's opponents are rendered mostly insignificant, owing to misunderstandings or even to malice. When the debate received increased attention in the 1960s, these views still prevailed (Westfall 1966a, 1966b; Sabra 1967). Although early in his career Kuhn also analyzed Newton's optical papers (Kuhn 1958) his impact on the historiography is mostly due to his disciplinary influence. A new stream of scholarship in history of science either challenged Newton's superiority in the debate (Bechler 1974; Burwick 1986), or further refined and developed earlier insights (Shapiro 1980; Westfall 1980; Bazerman 1986).

The new readings problematized entrenched views on Newton's style, methodology, and results. Bechler, for example, wrote the following about the style of the 1672 paper, which was traditionally regarded as exemplary:

The first paragraph of Newton's 1672 optical paper announced the need for a revolution in optics. Three pages later, the revolution was performed, again in a single short paragraph. The rest of the paper carefully explained that all was over: The revolution was complete ... This was bad form. One doesn't just walk in, announce a fundamental inconsistency in accepted scientific beliefs, declare the need for a revolution, perform it, and walk out. Things are simply not done this way.

(Bechler 1974: 116–117)

Newton's methodology, most notably the demonstrability of the famous *experimentum crucis*, was also found problematic (Laymon 1978a, 1978b; Gruner 1973; Sabra 1967). This obviously affected the appreciation of the other correspondents, which was especially problematic for Lucas from the Liège group of Jesuits, as his data (if trusted) could have led to the discovery of achromaticity, yet many historians even questioned his ability (and willingness) to replicate experiments and publish reliable results.

Differences in historiographical position also contributed to fundamentally diverging accounts. The historiography of the episode shows that historians disagree about the exact positions of the actors as well as about the evaluation of their moves. There is no consensus about the significance of various phases in the debate, as evidenced by the conflicting narratives of Schaffer and Shapiro (Schaffer 1989; Shapiro 1996). In short, while scholars agree that Newton's new theory was significant, and sparked an important controversy, agreement stops at this point. That such disagreements are expected to arise in the process of reconstructing controversies, as I argue below, stems both from the reconstruction of the argumentative use of methodology and from the evaluation of the correspondents by the historian, directly influencing the significance of 'minor' contributors in the resulting narratives. I begin by looking at how certain argumentative moves are connected to the methodology of a participant in a scientific controversy, and how these are reconstructed.

3. The methodology of reconstruction and the position of the protagonist

When invited to contribute a paper to a volume addressing the question of rhetoric in science – overlapping in scope and partly in contributors with the present volume – Richard Westfall, Newton specialist and author of Newton's definitive biography opened his article with a clear statement on his approach:

I have never formally studied the discipline of rhetoric ... I employ a wholly intuitive, common sense understanding of the word. ... As the author of several books, I cannot remember having ever systematically attempted to define the audience to which I was writing, and I tend to suspect that neither Galileo nor Newton consciously tailored their books to specific groups. On the other hand, I have always written in the knowledge that there is an extensive community of historians of science, as well as cultural historians, scientists, and others, whom I can expect to be interested in studies of science in the seventeenth century. I assume then that Galileo and Newton, in the same way, without consciously defining an audience, wrote in the knowledge that there was someone out there to address, and that they adopted rhetorical strategies suited to their respective scientific communities. (Westfall 1991: 107)

This is an admirably honest and revealing comment. Although seventeenth-century scientists had a much more thorough schooling in rhetoric than most twentieth-century historians of science, it appears very hard to determine the exact role rhetoric played in the production of texts for members of a tradition overtly scoffing at rhetoric and stating that they aim “to overcome, not an adversary in argument, but nature in action” (see Pera 2000). Clearly, they did not always write about their research the way we think they did it, but few scholars would hold today that speaking publicly has no effect on the way scientists express themselves. Doing science and writing science are different activities, as Schickore succinctly claimed: “Evidently, historians, philosophers, and sociologists of science of different stripes acknowledge that there is a mismatch between what scientists do and what they state they did” (2008:334).

This is disconcerting. We treat methodology on the one hand as a way of acquiring knowledge but on the other hand as an important argumentative resource of the scientist, that influences the acceptance of a position. If researchers fail to do what they preach, is this discrepancy in itself epistemically important? Such questions, which derive from the “mismatch” between research practice and verbal utterances, can be answered in part by finding the function of the discrepancy. Writing science *might* be connected to how research is done, but it is *evidently* connected to how research is received. Methodology informs ways of acquiring knowledge, but when used in debates it also has argumentative functions. If an actor's methodology can become an important part of the actor's argumentative

strategy, then methodological developments can be closely linked to developing argumentative resources. Newton's remarks on his position and the way he defended his theory from objections is a case in point.

3.1 Defending a theory from objection via methodology

In a letter to Oldenburg, later published with some abbreviations as the "New Theory of Light and Colours" (Newton 1671–72), Newton seems very open to criticism of his discovery, inviting the Society to perform the experiments, and "That, if any thing seem to be defective, or to thwart this relation, I may have an opportunity of giving further direction about it, or of acknowledging my errors, if I have committed any" (Turnbull 1959: 102). Newton's openness, however, appears to dwindle already in the early phase of the controversy, as he writes:

And therefore I could wish all objections were suspended, taken from Hypotheses or any other Heads than these two; Of showing the insufficiency of experiments to determine these Quere's or prove any other parts of my theory, by assigning the flaws and defects in my conclusions drawn from them; Or of producing other Experiments wch directly contradict me, if any such may seem to occur.

(Turnbull 1959: 210; Newton 1672b: 5005)

This is not the sole locus during the debates where Newton appears to use similar tactics. To the Jesuit Pardies, another correspondent in the early part of the controversy, Newton wrote:

...if anyone makes a guess at the truth of things by starting from the mere possibility of hypotheses, I don't see how to determine any certainty in any science; if indeed it be permissible to think up more and more hypotheses, which will be seen to raise new difficulties.

(Newton 1672a: 4014 for Latin original;
see concise translation in Turnbull 1959)

Newton rejects certain challenges to his position, such as objections that are derived from hypotheses, and provides *arguments* to support this rejection. These can be read as methodological remarks, and as such they prefigure one of the best known elements of Newtonian methodology, the famous "*hypothesis non fingo*" maxim, the objection to framing hypotheses. They can, however, just as easily be treated as argumentative moves with specific functions. Rejecting certain types of objections,⁶ like the consideration of rival explanations (hypotheses) proposed by critics, is a potentially successful way of defending one's own position.

6. Maurizio Mamiani, analyzing the early parts of this debate even suggests that "objections" here refers to the canonical figure discussed in Aristotle's *Rhetorics* (Mamiani 1991: 159).

When reading these quotes, can one decide whether they show Newton's methodological development or whether they are part of his argumentative strategy. In the case of a consistent use of methodological notions during debates the distinction seems to collapse⁷. Likewise, a gradual shift in position can be interpreted as the development of either methodology or argumentative strategy. It is only when the historian sees inconsistencies, change of tactics, or 'ad hoc' arguments, that these loci cease to function as 'methodological', and instead become merely 'rhetorical',⁸

It is now a commonplace that when scientists defend their positions against different parties – as when they are involved in 'boundary work' – they readily change their patterns of arguments. In this specific controversy, Newton appears to be flexible when it comes to giving reasons for rejecting objections. When, instead of suggesting rival hypotheses, the Jesuit Lucas in the later part of the controversy challenged Newton's results and experimental approach, citing a list of experiments, Newton again refused to take up the challenge by advancing *different* methodological norms.⁹ The methodological norm Newton used here was connected to his use of the crucial experiment in the aforementioned letter to the Royal Society. Commenting on the experiment with two prisms in a darkened room he stated: "Now if this demonstration be good, there needs no further examination of ye thing; if not good the fault of it is to be shewn, for ye only way to examin a demonstrated proposition is to examin ye demonstration" (Turnbull 1960: 79). As Lucas raised the type of objection Newton was previously asking for, Newton's move further restricted the kind of objections he was willing to consider and to which he was willing to respond. His argument in support of this rejection was the longest piece of argumentation up to that point in the whole controversy. I have analysed this elsewhere in more detail (Zemplén 2008), but a short quote is representative of many other loci:

7. Or if the same notions reappear in notes clearly not intended for publication, etc.

8. In Gieryn's influential approach, the 'boundary work' of scientists is analysed, and the rhetorical strategies to stabilize or change existing disciplinary or interdisciplinary boundaries are highlighted (Gieryn 1983). The present analysis, however, traces such changes in cases that would usually be treated by 'internalist' as opposed to 'externalist' approaches.

9. Lucas clearly tries to comply with Newton's demand, to present contradictory experiments, instead of rival hypotheses: "The experimentum Crucis then in my opinion, requiring further assistance I conceive, the most effectual means to strengthen it, and jointly to evince that *rays differently coloured, are also differently refrangible of their own nature*, would be to make a *Number of experiments*". Various versions of the letter survive: this – later – copy is transcribed in Turnbull (1960: 247). When Newton declines to respond to the challenge backed by experiments, Lucas clearly sees it as a "digression", exclaiming: "I having but follow'd the way which he himselfe had tracked out for me". (Turnbull 1960: 190).

whereas should I be drawn from a demonstrative experiment to begin with those [experiments that Lucas proposes to test Newton's theory], it might create us both ye trouble of a long dispute, & by ye multitude of words cloude rather than clear up ye truth. For it has already cost us so much trouble to agree upon a ye matter of fact in ye first and plainest experiments & yet we are not fully agreed: what an endless trouble might it create us, if we should give our selves up to dispute upon every argument that occurs, & what would become of truth in such a tedious dispute.
(Newton 1676: 703; Turnbull 1960: 80)

Already these loci draw attention to recurring features of scientific controversies: responding to challenges is regularly refused via invoking methodological norms, and these methodological norms are frequently embedded in argumentation¹⁰. By not responding to objections directly and by invoking methodological notions to justify this practice, the protagonist (in this case Newton) proposes procedural norms that decrease his burden of proof. The antagonist either accepts these norms, in which case his objection has been left unanswered, or takes up the challenge and maintains his objection, but in this case he is also required to argue against the procedural norms proposed by the protagonist. This in turn channels the disagreement from a ground-level dialogue into metadialogue(s), dialogue(s) about the ground-level dialogue (Krabbe 2003).¹¹

3.2 Methodology as argumentation

Evidently, differing interpretations result when interpreters focus on either the methodological or the argumentative role only. The methodological reading paves the way for an understanding of Newton's methodology; a topic that can be philosophically appreciated or criticized, it can be historically traced back to earlier writings. It is a theme that has been investigated and debated for centuries, with the aim of reconstructing what Newton's position on methodology was.¹²

10. I do not give here a definition of what argumentation is, and various approaches treat differently the extension of the term 'argumentative'. To avoid difficulties at this point I simply think of a text as argumentative, when approximately ten lines of it could appear in any textbook on argumentation as an example of some form of argumentation.

11. In the tripartite division of discussion-controversy-dispute by Marcelo Dascal, controversies are typically seen as fruitfully functioning on this meta-level. Discussions do not rise to this level, while disputes become polemical and abusive (see Dascal 2008). In Finocchiaro's notion of meta-arguments, much of the methodological level of controversies starting from ground level (e.g. discussion of empirical findings) can be reconstructed as meta-arguments (Finocchiaro 2007).

12. This approach generally reconstructs 'internalized' positions. In argumentation theory internalized versus externalized positions are discussed in e.g. Eemeren and Grootendorst (2004).

The argumentative reading, on the other hand, yields an analysis of the strategic use of methodological notions: how is a position defended persuasively or how parties can be convinced to accept a position.¹³ This reading enables us to reconstruct the role of methodology in Newton's rhetorical strategy, and understand some changes of it. The argumentative reading provides a context-sensitive interpretation of the argumentative toolbox of scientists who employ methodological views in their argumentative practice.

But methodology and argumentation do not oppose each other. Mamiani, also taking examples from the controversy discussed here, argued convincingly that for a methodological reconstruction these appear to be the "face of the Newtonian search of certainty", when analysed as elements of the argumentative toolbox of a scientist appear to be strategies "forbidding objections" (Mamiani 1991: 167). The same holds true for a significant number of other loci, not only the ones discussed with reference to these early optical controversies. A most obvious example is Newton's last *regula philosophandi* of the *Principia*, easily interpretable as both argumentative and methodological. The rule states that "In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions". While often cited as a key element in Newton's methodology, it is as much a traditionally understood methodological rule 'for the study of natural philosophy', as a methodological rule that functions as a procedural norm aimed

13. As a member of a community, every participant has ideas and expectations about the community's standards, about the ways other members of the community could be persuaded to change their views. Clearly, in the seventeenth century, scientists *qua* writers were well aware of the fact that their written work is intended for a reading public. But it is here that the agnosticism of Westfall might serve as a warning. It already appears problematic to reconstruct Newton's views on methodology: the received view in any period on Newton's methodology tends to be influenced by the received view on the methodology of the period, and far too many attempts have been made to claim that Newton's methodology is close to that of the historian, from Whewell (1837) to Worrall (2000). For a critical evaluation of some of these attempts focusing on Newton's practice of unification see Ducheyne (2005). The problem of reconstructing Newton's views on rhetoric, and how he tried to *consciously* develop strategies is plagued by the same difficulties involved in reconstructing his methodology, but it appears to be an even harder task, as Newton explicitly addressed many methodological issues – even if these had argumentative functions – while he never explicitly discussed his position on rhetoric. Such a reconstruction would still be aiming at an 'internalized' position (reconstructing what Newton thought about rhetoric). It is, however, also possible to view these statements as parts of an 'externalized' position, to reconstruct and analyze the speech acts Newton used in a debate. Such a reconstruction appears less problematic and is also more compatible with modern approaches in science studies (Kutrovátz 2008).

at rejecting certain types of objections. This is made very clear by the explanation of the rule: “This rule should be followed, so that arguments based on induction may not be nullified by hypotheses” (Newton 1999:796).

This argumentative aspect of methodology means that in argumentative situations we can ascribe certain functions to methodological notions. Thus seen, every methodology, by virtue of stating methodological norms that can function as procedural norms in an argumentative situation, can delimit the kind of objections that are considered legitimate, acceptable, etc. It can block challenges; it can be used to evade the burden of proof. In idealized models of debate it is a dialectical fallacy to discourage participants from stating their opinion or advancing an argument in a debate (Eemeren and Grootendorst 1987:285). In informal logic, not to allow perspectives and positions to be raised is often referred to as the *argumentum ad baculum* or appeal to the stick.¹⁴

This appears counter-intuitive at first sight. We generally see methodology as integral to and necessary for knowledge production, but we also think of fallacies as arguments that lead us astray and deter us from finding the best supported position on a given question. This raises interesting questions from the viability of ‘idealized’ situations from which some frameworks on argumentation start to rethinking the epistemic role journal publication structures might play, for instance; here I will focus on the burden of proof. Statements that make it difficult for potential critics to raise objections influence the burden of proof. The protagonist of a (novel) view is supposed to carry this burden, but by arguing for specific methodological norms (which restrict the types of objections) an antagonist has to take on the extra burden to challenge these norms, as well as raise objections. These methodological norms can be either just referred to or extensively argued for (thus distancing them from simple *ad baculum* arguments),¹⁵

14. For an overview of the *ad baculum* fallacy, including the recent upsurge in research, see Woods 1998. Although there are many rival ways of defining the fallacy, in most dialectical approaches many of the above norms given by Newton would be seen as fallaciously narrowing the types of objections to his theory, as most argumentation scholars agree that threats, uses of intimidation, etc., can be very subtle and implicit (like Pascal’s Wager). To claim that an objection arises from an improper method, that the opponent does not follow the proper ways and method of science, is a way of intimidating an opponent. In pragma-dialectics the ‘right to challenge’ is Rule 2, and *ad baculum* arguments are discussed within Rule 1. But the ‘Freedom rule’ is also relevant for the right to challenge (see Eemeren and Grootendorst 2004: 137–138, 190).

15. Methodology as argumentation is also methodology as institutionalization. The convincing methodological notions themselves became integral to the structure of science, influence how an article is to be written, for example. This is why the ‘mismatch’ is most pointedly put forth by followers of rival methodologies. See Goethe’s or Medawar’s attack (the latter discussed in Schickore 2008).

but the new (scientific) views are buttressed with argumentative/methodological scaffolding, which helps shift some of the burden of proof, and make a position easier to defend.

Although we focused on Newton's attempts to reject certain types of challenges, when similarly instructed by opponents, his response is very telling. As he wrote to Hooke, who "knows well yt it is not for one man to prescribe Rules to ye studies of another, especially not without understanding the grounds on wch he proceeds" (Turnbull 1959: 172). This comment is made on Hooke's methodological remark, and shows that Newton was just as sensitive to being restricted this way as he was innovative in restricting others.

To sum up this section we may say that the 'scientific method', used to decipher the secrets of Nature can become a stratagem, when used in an argumentative context. And don't all scientific arguments presuppose an argumentative context? The Janus-face of methodology, discussed above, opens up a major and understudied problem space, bringing epistemological, historical, and argumentative perspectives together.

4. Understanding the position of the historian

The previous sections have shown that some loci can be read as *both* methodological and argumentative. The historiography of the controversy at hand, however, shows a predilection to interpret these as *either* methodological *or* argumentative/rhetorical. The first reading dominates when the methodological notions are applauded and usually coincides with downplaying the significance of other correspondents. As Westfall (1966b: 306) writes about Lucas: "his letters manifest a failure to comprehend the very nature of experimental investigation". If, however, the methodology is found wanting, it is more likely to be reconstructed as serving ulterior argumentative purposes. And, together with the argumentative reading, the opponents are generally seen in more positive terms. Thus Laymon calls Lucas' arguments "ingenious and bold" (Laymon 1978b), Sepper describes his critique as "sustained and well-planned" (Sepper 1988: 159), and in Guerlac's narrative he is "generally conceded to be the ablest of Newton's continental critics" (Guerlac 1981). The appreciation, whether positive or negative, of the participant in a controversy seems to go hand in hand with the reconstruction, stressing either the methodological or the argumentative aspects of the same loci. A short recapitulation of a recent theoretical debate on the previously mentioned *ad baculum* fallacies in the journal *Argumentation* can help us conceptualize this factor influencing the appreciation of Newton's methodological-cum-argumentative notions.

Kimball (2006: 93) has recently suggested that in order to evaluate *ad baculum* arguments, the consequences of a threat on the interlocutor's relationship with the speaker and the character of the speaker need to be investigated, as "acceptable and objectionable, benign and malicious threats cannot be distinguished by their form alone". The threat is likely to be benign in case the speaker's character is judged to be good, and these cases are not fallacious, but simply display good prudential arguing. An arrogant or narcissistic threatener, however, is likely to perform a malign threat (ibid.: 96). The prior negative evaluation of the speaker's character thus becomes a crucial criterion when evaluating these arguments.

Kimball's criterion of the abusive *ad baculum* argument, however, has been criticized as a problematic practice of argument analysis (Walton and Macagno 2006). Evaluating the speaker's character belongs to virtue ethics. But judging the "intentions, purposes, and character of threateners" (Kimball 2006: 96) is not what argument analysts (or for that matter historians) would necessarily excel at. While their criticism is well argued, the procedure explicated by Kimball nevertheless captures certain psychological processes that can influence the evaluation of *ad baculum* type arguments. This provided, the analysis of most historians addressing the specific controversy can be understood using Kimball's framework. As Kimball would suggest, people who generally have a positive appreciation of Newton – like scholars spending much of their academic careers preparing biographies of Newton or critical editions of his works – would tend to see Newton's arguments as benign, only helping the scientific inquiry (for examples see Zemplén 2008). If, however, historians spent considerable time and effort studying the writings of Newton's opponents, they are apt to discuss the previous notions as malicious. In recent historiography, such a cluster is formed by sympathizers of Goethe's scientific writings (Gruner 1973, 1974; Sepper 1988). Goethe himself studied – and sided with – Newton's opponents in the debate discussed here; in fact he was the first to praise Lucas' critique of Newton. Studying other opponents of Newton, like Hooke, appears to trigger a similar reading. Lisa Jardine, in her recent biography of Hooke notes that "The scholarly work on the controversy [between Newton and Hooke], generated almost entirely out of the Newton 'industry' which has dominated history of science research since Newton's death, is hopelessly partisan" (Jardine 2004: 326).

Obviously, Jardine's comment itself can be considered partisan. But my point is not intended as a criticism, nor is it a strict categorization of historians. Yet these examples can direct attention to a factor influencing the historian's reconstruction. While virtue ethics has been shown to be a problematic starting point for evaluating *ad baculum* arguments, evaluation of character appears to influence historians' positions on the methodological notions used by the participants of a controversy, and seems to be connected to their specific 'socialization' in the

field.¹⁶ Taken seriously, this means that not only the analytical framework influences the reconstruction of these texts (like in the aforementioned debate between Shaffer and Shapiro), but also the attitudes of historians towards the participants. To connect this to the earlier analysis, one can plausibly generalize that seeing loci as benign threats yields a ‘methodological’ reading, while the negative evaluation of the speaker paves the way to present the same utterances as argumentative stratagems to silence objections. In either case, the reading can be considered slanted, and the historian should be careful since the appreciation of character can inform the evaluation of the position.¹⁷

Not only disciplinary framework and prior evaluation play a role in how controversies are discussed. Next I turn to the volatile issue of how social factors – as opposed to natural ones – are accounted for in analyses of scientific controversies, and whether these justify (and if so, how) special attention to the influence of the argumentative situation. Accepting that some of the novel methodology of the Scientific Revolution has had argumentative functions is a weaker claim than the suggestion that the argumentative situation influenced the *invention* of modern methodology.

5. The natural, the social, and the argumentative

Some claim that in history and philosophy of science it is “widely maintained that the methodology to which a scientist subscribes is really little more than perfunctory window-dressing, which is honoured more in the breach than in the observance” (Laudan 1977: 58). The negative lesson drawn from this, most commonly attributed to Feyerabend, is well-known. Yet the analysis of the argumentative role of methodological norms has the potential to highlight the functional relevance and possibly the epistemic function of this *discursive* use of methodology. While not common practice, this does not go against common contemporary approaches.

16. Socialization and bias deserve a significantly more detailed discussion. Here I only refer to modern epistemological approaches that do not draw a sharp line between hot and cold cognitive factors (Solomon 2001), based on studies of biases contributing to positions.

17. In recent Science Studies literature – along different lines – this has clearly been recognized (Scott, Richards and Martin 1990). I aim to show, however, that the danger is also imminent when analysing non-contemporary controversies. As Kincaid rightly observed: “If social processes need to be taken into account in evaluating physics, it should be no surprise that they need to be taken into account when assessing the research and reasoning processes and results of scientific historiography” (Kincaid 2009: 302).

Today, after many rounds of debates, historians are well informed, conscious, and cautious about their explanatory and narrative strategies. Our present understanding of Nature plays a limited role in the explanations of earlier views; in fact the role Nature plays in narratives in history of science has decreased in recent decades. As the significance of social considerations in historical narratives increased substantially, the social dimension has been used not only to account for views on nature, but also to explain how arguments are used. The evaluative edge that characterizes much of argumentation theory rarely surfaces in historical accounts, and factors external to the argumentative situation are generally underscored when argumentative utterances are accounted for or explained.

Nevertheless a fruitful line of mostly rhetorical research investigated the argumentative practices of scientists. Yet this important body of scholarship has often presupposed the significance of the argumentative, and studies rarely argued for it. To help decide whether the 'omnipresence of debate' had a constitutive role in what was to become a new approach to knowledge and rationality in the Scientific Revolution, one option is to investigate whether among the other elements of the social sphere one can find a distinctive and separable 'argumentative sphere'. Once the existence and independence of this is determined, its precise role can be evaluated. A persuasive illustration of the separability of the argumentative sphere comes not from a specialist in argumentation, but from Michael Billig, a social theorist. Introducing and commenting on his example below, I will argue for the distinctive role argumentative situations played in shaping positions, and by derivation, in the development of modern science. The example will further be used to introduce another crucial issue, the way positions are reconstructed 'charitably' when studying debates. This in turn is shown to influence the position of controversies in the historiography of early modern science.

Using a transcript of a conversation between a man and a woman carrying her dissertation to the conversation, taken from cognitive scientists (Hobbs and Evans 1980), Billig stresses the importance of the argumentative situation for the analysis of utterances. In the original article, the cognitive psychologists analyze the conversation according to social goals ascribed to the participants, not unlike recent sociologically inspired work in the history of science, analyzing actions with reference to interests or goals. Yet when an utterance seems to be at odds with the goals the psychologists posit, they do not analyze this comment. As Billig notes "If anything, this outbreak of irrelevant, and rather artless, witcraft seems to threaten the success of plans" (Billig 1989:86). The interchange "drifted into argumentation", and the utterance could not be accounted for as a means to an end, as a social act aimed at reaching the goals the discussant had when entering

the conversation¹⁸. As Billig notes, “Whether or not any resulting arguments are used to further ulterior purpose, they nevertheless possess their own dynamics. The rhetorical nature of these dynamics cannot be understood if argumentative interludes are seen as a means to other, principally non-rhetorical, ends, rather than being treated in their own right” (ibid.: 87).

As Billig’s example suggests, social factors (like institutional or socio-political factors), the local dynamics of smaller groups, or the interests and norms of participants might not be able to provide sufficient explanation for certain utterances. The macro- or micro-social explanations that historians of science generally utilize focus on interest and socialization and rarely consider the actual argumentative situation as the *cause* or motive of the specific action. Could the argumentative sphere provide an *explanans*?

While argumentation is clearly a social activity, the influence that the argumentative situation has on the positions put forth by the discussants can have its own dynamics. Separating the analysis of the influence of the argumentative situation from the influence of less localizable social factors in a controversy appears justified, and allows historians to investigate controversies and other argumentative exchanges in their own right.¹⁹

With respect to the controversy around Newton’s first publication, Newton responded to a number of letters from Lucas, but decided to suppress most of the correspondence. We can focus on the social goals Newton had in explaining this move. Yet the historian can also assume that the argumentative situation can elicit responses which were deemed unpublishable by Newton.²⁰

18. One could, of course, claim that at this point she had other minimally rational goals, e.g. ‘saving face’, but these goals are not the types of goals that sociological analyses of scientific controversies examine, but exactly the types of goals one has in a debate. Therefore I include these goals, which clearly have social functions, to be goals analyzable via the investigation of the argumentative realm.

19. This by no means suggests the independence of the two realms, but as such an analysis demands skills (discourse analysis, pragmatics, etc.) usually not present in the training of historians and sociologists, it is open to another field of expertise. As such, expertise in argumentation is clearly incorporable in modern approaches to science studies as a special case of meta-expertise (Collins and Evans 2007).

20. It could be further argued that Newton’s extremely popular inductivist methodology is partly a response to the challenges his notion of demonstrability received. As he wrote in the *Opticks*: “And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general” (Newton 1952: 404).

6. How to read charitably

Without acknowledging the possible role of the argumentative situation in shaping positions, it appears hard to see what constitutive role controversies might have had in the Scientific Revolution, in shaping the ideals and methodology of modern science. Granting that the argumentative situations *can* have their own dynamics but accepting that the evaluation of the participants of a controversy influences the historian's reconstruction raises the question of what is a 'charitable' way to reconstruct the positions, arguments, and even the methodology of scientists. I will argue that present day practices of 'charitable' reconstruction generally mask the role of the argumentative situation in controversies, but it is possible to see both the influence of the argumentative situation and to remain charitable.

To describe (mostly tacit) elements of the general practice I will use the transcendental/philosophical principle of charity as a proxy to highlight certain aspects of the typical method of reconstructing the views and beliefs of an author. This approach was famously explicated by Donald Davidson as a philosophical position, a universal principle that applies to any interpretation. In the standard reading of Davidson's work, the principle of charity is separated into two principles: coherence and correspondence. In various writings, Davidson expressed the need to credit people with a large degree of consistency, in his article on *Truth and Meaning* he stated that "Charity in interpreting the words and thoughts of others is unavoidable ... just as we must maximize agreement, or risk not making sense of what the alien is talking about, so we must maximize the self-consistency we attribute to him, on pain of not understanding *him*" (Davidson and Lepore 2006: 163). When commenting on his own method in the seminal essay *Radical Interpretation*, he explained that it "is intended to solve the problem of interdependence of belief and meaning by holding belief constant as far as possible while solving for meaning"²¹ (Davidson and Lepore 2006: 193).

This principle, when used to illustrate the practice of reconstruction in controversies highlights the importance of holding belief constant for an author, often producing several texts, and often in response to queries or criticisms from peers. The various loci are interpreted such that the speaker's beliefs have minimal inconsistency. This practice is in tune with the general historical or philosophical procedure, when a participant's worldview or position is to be revealed, explicated, summarized, and condensed.

21. Davidson is not the only one stressing coherence and consistency. Even for Kuhn who argued for radical differences in worldviews of scientists, consistency is also a recurring theme. Textual anomalies, are, for Kuhn, artefacts of reconstruction (Kuhn 1990: 299), and he highlights the role of coherence and consistency in the course of the reconstruction (Kuhn 1977: 17).

A corpus of texts by an author, however, is never by itself coherent and consistent, unless painfully repetitive. Coherence and consistency are mostly characteristics of the reconstructions.²² Let us return to the debate concerning Newton's theory of light and colors, and address an example that stresses this point. Before the controversy, Newton stated in his *Lectiones opticae* that:

The relation between the properties of refractions and those of colours is certainly so great that they cannot be explained separately. Whoever wishes to investigate either one properly must necessarily investigate the other. Moreover, if I were not discussing refractions, my investigation of them would not then be responsible for my undertaking to explain colours; nevertheless the generation of colours includes so much geometry, and the understanding of colours is supported by so much evidence, that for their sake I can attempt to extend the bounds of mathematics somewhat ... Thus although colours may belong to physics, the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning. (Newton 1984: 87)

This states that the science of colors can be mathematized, and, as explained in a famous passage from his letter to the Royal Society (omitted by Henry Oldenburg), this is closely connected to his *Experimentum crucis*, demonstrating the various refrangibility of light rays. Based on this discovery Newton states:

a Mathematician may determin all the Phaenomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in which severall colours are originally inherent; *I suppose the Science of Colours will be granted Mathematicall & as certain as any part of Opticks.* (Turnbull 1959: 187)²³

The conviction that Newton demonstrated a novel property of light, and that this property establishes a mathematical science of colors, reappears in the controversy following the publication of his letter as well as in the last lines of the various editions of the *Opticks*, Book II, Part 2 (see Newton 1952: 244).

22. This discussion is closely connected to a critical appraisal of Howard Stein's views on Newton's method (not discussed here), and is a further elaboration of a framework put forth by Zemplén and Demeter (2010), but connecting it to Davidson's views. For a discussion of rationality (and not just coherence) as a property of interpretations, see Demeter (2008).

23. Oldenburg also deleted the following passage from a later letter (21 September 1672): "To comply wth your intimation ... I drew up a series of such Expts on designe to reduce ye Theory of colours to Propositions & prove each Proposition from one or more of those Expts by the assistance of common notions set down in the form of Definitions & Axioms in imitation of the Method by wch Mathematicitians are wont to prove their doctrines" (Turnbull 1959: 237).

In his reply to Hooke, however, Newton wrote that his notion was not of “geometrical demonstration” (Turnbull 1959: 113). Nevertheless, he claimed that it was certain to the extent any premise in physics was certain, in short, as certain as the other mixed mathematical sciences.

Already these passages suggest some wobbling, and a unified position is only reconstructed by following certain (implicit or explicit) methodological norms of reconstruction. When a reconstruction of a unified position – that is reconcilable with all the loci – is reached, the significance of slight discrepancies disappears, and these become variations on a single theme, helping the historian to reconstruct Newton’s beliefs.

Thus far there is no serious reason to abandon this method of reconstruction. The utterances can be reconstructed so as to yield a coherent and consistent position. Yet, in the heat of the controversy, Newton appears to give up his conviction that the secondary quality of color can be the marker of a primary quality²⁴ and with this the possibility to mathematize colors. Responding to the already mentioned experimental objections raised by Lucas, he writes:

[The Question of different refrangibility which I bring] ye Experimentum Crucis to decide, is not, as I sayd whither rays differently coloured are differently refrangible, but only whether some rays be more refrangible yn others ... If you consider this you wil see yt while you have laboured to oppose different refrangibility, there’s not one of your objections wch concerns it except ye third where ye Experiment succeeds otherwise than you have reported it. The rest are only against analogy between refrangibility & colour, for *if I would say yt rays differently refrangible have no appropriate colours, all your objections would cease.* (Turnbull 1960: 257–258)

By giving up one of the most cherished elements of his theory, the correspondence between refrangibility and color, Newton renders Lucas’ objection irrelevant.²⁵ This instance appears to be a clear case of the argumentative realm influencing one’s position, if one had doubts about the argumentative situation having its own dynamics. The participant’s utterance appears to be at odds with his general goals, as Newton forbade the publication of this letter, and apart from the first exchange between him and Lucas, suppressed the correspondence.

24. This basic tenet also reappears in the *Opticks* (see the Propositions and Queries 12–13).

25. Newton endorsed the mathematization of colours in various writings, both written before and after the controversy. During the controversy, however, he supported his thesis with reference to his crucial experiment and the propositions. In his response to Lucas, he changed the function of the crucial experiment in his reasoning, and with this he gave up his only publicly available argument in defence of the thesis that refrangibility determines colour (and vice versa). He later reiterated the thesis, this time without referring to the crucial experiment (Newton 1952).

If, however, the argumentative sphere does play a role in shaping the position of the actors, then it is not only consistency and coherence that should guide the reconstructions; attention should also be paid to the micro dynamics of the argumentative situation. Focusing on the local argumentative context is in fact what most argumentation theorists call the ‘principle of charity’. Inconsistency and incoherence in a corpus is plausibly ascribable to the participants, if we see the utterances as responding to the (various) local argumentative situations.²⁶ While in standard (analytical) philosophical practice the principle of charity is used to minimize and potentially discard the apparent inconsistencies and incoherence, the frameworks in argumentation theory generally take the argumentative situation to be a potential constitutive factor in the development of positions. As leading proponents of the field recently summarized, analysts “take refuge to so-called ‘maximal’ strategies aimed at making the analytic choices that do optimal justice to the purposes of a critical discussion, but remain, in fact, arbitrary – as discussed under ‘maximally reasonable reconstruction’, ‘maximally argumentative interpretation’, and ‘maximally argumentative analysis’”²⁷ (Emmeren and Houtlosser 2009: 2). These strategies all aim to maximize the argumentative force and relevance of the utterances, given their local argumentative context.

The two methods yield significantly different results. Historians prioritizing the former method of reconstruction (to which I will refer as ‘global principle of charity’) do not consciously search for inconsistencies, and even when finding them, are inclined to minimize their significance (Westfall 1980: 238). Even

26. While I reconstructed Kuhn’s position as one that is close to Davidson’s, clearly there are differences. And while I generally agree with Ian Hacking, that Kuhn’s hermeneutics is positivist (Hacking 1979: 226), Kuhn also lists “plausibility” as a guiding factor in reconstructions (Kuhn 1977: 17). If it is plausible that participants are influenced by the argumentative situation, then, of course, such changes in position become plausible. These in turn offer a more tenable hermeneutical position. Davidson’s work (especially his later essays) also affords a more flexible reading, especially if we take into account that he is conscious about the boundary conditions of his model, as he states: “the role of ordinary social arrangements has been totally neglected; the method assumes a society of two” (Davidson 2004: 35).

27. For both principles discussed here, I have chosen examples that are explicit, well-known in their respective fields, and symptomatic of the disciplines – Davidson and the pragma-dialectical school. Importantly, both principles select solutions which are underdetermined by the existing data. As Davidson wrote in *A Coherence Theory of Truth and Knowledge*: “Needless to say, there are degrees of logical and other consistency, and perfect consistency is not to be expected. What needs emphasis is only the methodological necessity for finding consistency enough” (Davidson and Lepore 2006: 235). As it is recognized in pragma-dialectics: “This predicament makes the analysis that can be achieved less thorough and comprehensive than desirable, its justification less firmly grounded than desirable, and an evaluation based on the analysis less well-balanced than desirable” (Emmeren and Houtlosser 2009: 2).

rhetorical accounts can maintain that the controversy influences not the content but the style only. Bazerman, in his account of the controversy only sees the development of “new rhetorical resources to answer criticisms” (Bazerman 1986:82), and no fundamental changes in position, only “some drawing back, some further elaboration, and further precision” (ibid.:84). Bazerman even claimed that the letters “required no rethinking or reformulation of argument, only a reiteration of existing statements” (ibid.: 116–117).²⁸

On the other hand, acknowledging the role the argumentative setup plays (even with respect to content) yields a reconstruction where the utterances of a single actor are not always reconcilable with each other. This ‘local principle of charity’ offers a more fragmented view of the actor’s position – the high mark of an uncharitable reading of philosophical texts or a sign of the speaker’s inferior intellectual abilities. Yet this reconstruction also acknowledges the ‘principle of charity’, but in a different form, where a charitable reading has to take (pragmatic and other) factors of the argumentative situation into account as well, even if this partly sacrifices a given position’s consistency and coherence.²⁹ In short, if the dynamics of the argumentative setup are taken seriously, inconsistencies in the corpus of the speaker’s utterances are normal and not exceptional. And this is not a sign of an uncharitable (or faulty) reconstruction, even if these inconsistencies in the historiography have generally been noted by historians not aiming to do justice (primarily) to Newton.³⁰

28. Most traditional philosophical reconstructions treat the rhetorical and literary aspects of a text as “a layer to be stripped away before concentrating on the arguments in the text” (Bicknell 2003:25). Traditional rhetorical analyses, on the other hand, often concentrate on the layer that is to be discarded. It could be argued that rhetorical approaches like Bazerman’s generally follow this ‘division of labour’ and shy away from addressing scientific content (and changes thereof), concentrating on the way this content is presented. A dialectical approach, on the other hand, is likely to produce a less monolithic reconstruction of the content as well, thus is more suitable for the analysis of the internal dynamics of controversies.

29. This is why I separated Davidson’s principle, and how it can be used to draw attention to aspects of a reconstructive practice. Clearly, even the ‘local principle of charity’ discussed here needs some orientation. As Davidson writes: “If we cannot find a way to interpret the utterances and other behaviour of a creature as revealing a set of beliefs largely consistent and true by our own standards, we have no reason to count that creature as rational, as having beliefs, or as saying anything” (Davidson and Lepore 2006:193). Newton’s behaviour, of suppressing the publication but later carefully adjusting his methodology to avoid the type of critique he received from Lucas calls for some universal principle of interpretation.

30. Registering inconsistencies and being charitable is not at odds. To cite Davidson once more: “From a formal point of view, the principle of charity helps solve the problem of the interaction of meaning and belief by restraining the degrees of freedom allowed belief while determining how

7. The consequences of 'radical dialectification'

These norms of reconstruction have not received sufficient attention in the historiography of controversies. I have provided an example where the argumentative situation appears to have significantly altered Newton's position. If such cases are to be found, as in fact I presume they are when looked for, then the reconstruction of scientists' positions should take these into account. This appears to rob us of the neat and homogenous positions of participants to which we are accustomed. This 'radical contextualization' (Zemlén 2008) or 'radical dialectification' renders positions much more context-sensitive (with respect to the local argumentative situation) than socializing approaches to history of science usually do. A multitude of commitments in various phases of a debate take the place of a participant's single uniform position.

As the arguments used are generally enthymematic – implying rather than explicating certain elements – the reconstruction of these implicit notions becomes significant. In many cases the reconstruction can do away with apparent inconsistencies; but to what extent should it aim to do so? The answer to this problem is crucial in providing an answer to the question raised earlier. To know whether the controversies had a constitutive role in shaping modern science, one has to know to what extent the argumentative situation shapes positions. But the methods used for the reconstruction generally presuppose the answer, circumventing its investigation.

Overlooking the influence of the argumentative sphere does not only affect the reconstruction of the protagonist's position; the role of antagonists is also influenced. To reconstruct a participant's position is often possible only after challenges were made and responses were formulated. Since it is the critique of the antagonists that triggers the responses (which, in Newton's case is a novel methodology with wide-ranging repercussions) the consequences are twofold. Not only is the contribution of the contemporaries downplayed, but their *ability* to contribute is also masked. This is most notable when 'minor' figures are investigated, such as Lucas (or Ignace Gaston Pardies) in this controversy. Diminishing the role of the lesser-known Jesuits when discussing Newton's methodological development means that much of the 'social construction' of the novel methodology is veiled, even in social constructivist frameworks (Zemlén and Demeter 2010).

to interpret words" (Davidson and Lepore 2006: 234). The 'local principle of charity' restrains the 'degrees of freedom allowed belief' less, as it treats the argumentative setup as influencing the positions. With this, however, the two reconstructions characterised here presume different norms of rationality. While this is not explicated here in detail, it is to be noted that modern cognitive research into rationality and heuristics is more compatible with the 'local' reconstruction.

This is relevant not only for our understanding of the controversies in the Scientific Revolution, but also for our general understanding of science, and the *invention* of novel methodology in any period. If only means of persuasion change during scientific controversies, but positions themselves do not, we are left with a rhetorical account of belief-change, where acceptance is explained with recourse to persuasiveness.³¹ But a dialectical model utilizing the ‘local principle of charity’ allows us to find changes in the positions and to connect these to the challenges raised by discussants, thus seeing them as (mostly reasonable) attempts to defend positions in light of various objections. To borrow the words of Marcello Pera outlining his similar, ‘dialectical’ project:

If it were possible to build the missing bridge between persuasion and scientific knowledge, then one would be in the best position for overcoming the tension between “internal” and “external”, and between normative and descriptive philosophies of science. A dialectics *proper* to science ... should be able to show how external factors become internal and how internal factors are conditioned by external ones. (Pera 1994: 11)

Taking seriously the argumentative role that methodological notions play in controversies (i.e. looking not only at their epistemic but also their persuasive and justificatory role) and reinvestigating our traditional approaches to reconstructing scientists’ positions, are crucial steps in building this missing bridge.

8. Conclusions

By drawing attention to the attitudes of analysts of controversies, one can transcend and explain much of the disagreement that characterizes their historiography. In Newton’s case I have sketched a plausible interpretation as to why some of these accounts – describing but also evaluating protagonists’ moves – are so radically different. Secondly, I characterized two methodological approaches that both aim to provide a ‘charitable’ reading of utterances. Highlighting the differences, I aimed to show that the prior methodological position of the historian influences

31. Changes of position are accepted in many cases, especially when they are explicitly stated (as illustrated by e.g. the conversion of Hilary Putnam), or when they occur over longer periods. My aim was to discuss short-term changes in externalised positions, as they are affected by the argumentative situation. In Newton’s case, who, in this period, carried out little empirical work that would change his position in the debate, this is all the more striking. (His ‘Hypothesis’ and ‘Observations’ read at the Royal Society in this period do not directly relate to the change discussed here).

the answer to the question whether the controversies have a constitutive role in shaping positions and the received view emerging from the debates or not.

Concerning the specific case study, the debate around Newton's "New Theory of Light and Colours", we have seen how the argumentative sphere can be seen as affecting the position Newton endorsed. These examples showed that the *content* of scientific theories can be shaped by the dynamics of the controversy. Argumentative situations also facilitate the use of methodological norms as argumentative devices. Elements of what is considered to be 'Newton's method' become utilized in the controversy to reject certain types of challenges. This opens up ways to discuss methodology as more than "perfunctory window-dressing, which is honoured more in the breach than in the observance", as Laudan put it, in cases where it is not easy to connect methodology to the actual research-practice. The analysis of methodological norms as argumentative tools in dialectical situations enables the 'radical dialectification' of scientific method, allowing us to see much of the development of modern science and the scientific method in a new light, as a socially constructed and 'reasonable' enterprise. While the epistemic significance of methodology has long been acknowledged with respect to the practical side of science, the argumentative uses of methodology point to another, epistemically relevant, aspect of methodology, its use in debates, and the role it plays in criticizing or defending positions.

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PART III

Physiology and vitalism

Salient theories in the fossil debate in the early Royal Society

The influence of Johann Van Helmont

Anna Marie Roos

1. Introduction

In the late seventeenth century, several luminaries in the early Royal Society debated the origins of fossils. Past historiography, particularly the work of Martin Rudwick and Rhoda Rappaport, has sketched the broad outline of the controversy (Rudwick 1985: 61–65, 81–88; Rappaport 1997: 105–106). The naturalist John Ray (1627–1705) and microscopist Robert Hooke (1635–1703) argued that fossils were remnants of past animal and plant life, although they differed in opinion of “how fossil-bearing strata had been deposited” (Rappaport 1997: 106). Since the Christian creed taught that all species were created in Genesis, any ideas concerning the extinction of the species were considered heterodox. On the other hand, Robert Plot (1640–1696), secretary of the Royal Society and keeper of the Ashmolean Museum, and Dr. Martin Lister (1639–1712), the first arachnologist and conchologist, stated that fossils were not always remains of living creatures, but could be created spontaneously by nature as part of her inherent “generative powers” (Roos 2008: 350). They represented the views of early modern naturalists who postulated that metals and minerals were spontaneously nurtured and generated in deep mines considered to be Mother Nature’s womb, and stones that resembled living creatures could be generated without any organic origins. In other words, although we think of fossils as remains of living creatures, many seventeenth-century investigators thought these stones could be created spontaneously by nature as part of her inherent “generative powers”. Intriguingly, other stones curiously wrought by nature, such as trilobites, had been found that did not resemble any existing living creatures, giving weight to the idea that fossils were *lapides sui generis* or merely “formed stones”. Underground aquifers carrying waters with generative seeds were thought to form rocks and minerals in the heat below ground. Mineral-generating seeds in the atmosphere could also be responsible for fossilisation.

Little work has been done examining Lister's and Plot's explanation of these "generative powers" and "seeds" which this article will show were all differing adaptations of or reactions to Belgian physician Johann Van Helmont's (1579–1644) theories in saline chymistry.¹ Antonio Clericuzio (2000: 52) has noted that several members of the Royal Society in the late seventeenth century developed and transformed Van Helmont's chemistry by interpreting its main notions – *semina*, ferments, spirits and the Alkahest – in terms of corpuscles, particularly *saline* corpuscles endowed with a "plastic formative power". The term "salt" or "saline" in the early modern period was a vague one, but usually described solid, soluble, inflammable substances with characteristic tastes and a crystalline structure. In the early modern period, as historian Norma Emerton (1984: 214) has commented, "as the instrument of the form, as embodiment of the generative seed and spirit, and as the transmitter of mineral qualities including crystallinity, salt became the formative principle par excellence". There has been no analysis, however, to what extent these interpretations of salts affected the fossil controversy over their formation, composition and nature. This paper will thus elucidate the disagreements amongst Lister and Plot about the fossilisation process, placing their work in the larger context of seventeenth-century natural history and Helmontian chymistry. Their debate over the origin of crinoid or sea lily fossils will be utilised as a case study to elucidate the fossil controversy and its chymical basis.

2. Context

A major feature of early modern natural philosophy was the rapid and exponential growth of natural history; as Lorraine Daston has indicated, "it would be more correct to talk of a proliferation of natural histories ... all with different philosophical pedigrees" (Daston 2000–1: 1). Part of this renaissance of natural history involved the creation of detailed chorographies to describe the natural landscape, its flora, fauna, and its geology. Nicholas Steensen or Steno (1638–1686), wrote a chorographic work of Tuscany, the *Prodromus* (1668), which was a bold assertion of the organic origin of fossils and how they came to be enclosed in layers of rock. He described a process in which juices seeped through cracks in the earth that were caused by the movement of geological strata. This juice dissolved mineral

1. My use of the term chymical and chymistry throughout this essay is quite deliberate, as it is anachronistic practice to make clear distinctions between alchemy and chemistry in the seventeenth century. Early modern "chymists" attempted to transmute metals into gold, considered an alchemical practice, yet also performed other experiments involving mass balance or crystallographic analysis that would be considered "chemical" (see Principe and Newman 1998).

salts, penetrating interstices of animal shells, eventually replacing the shells with a stony substance. In his speech in 1673 at the Copenhagen Anatomical Theatre, Steno rejected the ancient authorities in natural philosophy, proclaiming: “Fair is what we see, Fairer what we have perceived, Fairest what is still in veil” (*Pulchra sunt, quae videntur pulchriora quae sciuntur longe pulcherrima quae ignorantur*; Steno 1994: 118). It is traditionally asserted that Steno formulated the first laws of stratigraphy from his observations in Tuscany described in the *Prodromus*.

In the 1660s, however, Steno also had been in Montpellier where the geological environment has been described as “privileged”, as the city itself lies on Neogene sub-horizontal layers that contain visible outcrops of fossilised oysters in the botanical garden and city gates (Ellenburger 1999: 104). The surrounding countryside had other fossil treasures – enormous oysters and *glossopetrae* (teeth in fossilised forms) – which were the source of Steno’s conclusion that fossils were the remains of extinct organisms. He noticed the similarities between modern shark teeth and their fossils. Lister was pursuing his medical studies in Montpellier at the same time, and going on “simpling” expeditions with John Ray to collect natural history specimens. Upon his return to England, Lister remembered that on “the shores of the Mediterranean Sea, there may all manner of Sea shells be found promiscuously included in Rocks and Earth, and at good distances too from the Sea” [Montpellier is 10 kilometres from the sea] (Lister 1671: 2283; Ellenburger 1999: 104). Lister and Steno did not just share geological interests. Steno, already known for his discovery of the duct of the parotid salivary gland (Steno’s duct), performed a series of four dissections with Lister’s assistance in the study of Robert Bruce, the First Earl of Ailesbury and Second Earl of Elgin. Lister praised Steno’s technique which was “neat and clever” and admired his “genius and great personal modesty”, stating “I observed in him (very much) of ye Galant & honist Man as ye French say, as well as of ye Schollar” (MS Lister 5: 224–226; Iliffe 1998: 376). It seems quite possible that the “seeds” of Steno’s and Lister’s ideas about the origins of fossils may have been planted during their time in Montpellier, though they would eventually disagree.

3. Martin Lister and his theory of fossils: A refutation of Helmont

Lister was the first conchologist, and his publication of a chapter about fossilised conches in his *Historiae Animalium* was the first attempt to create a comprehensive account of fossilised molluscs (Arnold 2005: 213). The fossils in the *Historiae* were arranged according to their form, his classification similar to those characteristics he used to classify living molluscs; the number and form of valves, position of the hinge, number of turns of the spiral of univalves, and the presence or absence of

a spine and umbilicus in ammonites were all identifying traits (Carr 1974:281). As the fossilised conches had different features from extant species, Lister thought they were most likely formed stones; Lister, for instance, successfully confronted Robert Hooke at a Royal Society meeting concerning what Hooke called “petrified oysters” (Birch 1756–7, vol. 4:237–238; Carr n.d.:21). Lister countered that these specimens had “no striae going from the valve to the rim”, as had living species of European oysters (Birch 1756–7, vol. 4:237–238; Carr n.d.:21). On the other hand, in his *Historiae Animalium*, Lister also noted, after claiming fossils were mere “formed stones”, though he did not completely “disregard the fact that these are much like living things of which nature has wearied. Certainly I have thought about these possibilities, especially the influential doctrine of my greatest friend John Ray” (Lister 1678, Book 3:1). Lister followed this comment by stating that he would “stop these ruminations in the presence of the reader; they [the specimens] may speak for themselves. If yet it is able to be judged what these earthly stones are to be, I will consider it, nor will I make rash judgements” (Lister 1678, Book 3:1). Lister even pointed out that on some fossil specimens, there were worm tubes on the surface or fossilised pearls, which might indicate that their origins could be from living creatures (Carr n.d.:27). Lister’s equivocal comments led the geologist Charles Lyell in his *Principles of Geology* (1830) to note that Lister was one of the first to consider the extinction of species, which was true; but then he went on to say:

that Lister and other English naturalists should long before have declared in favour of the loss of species, while Scilla and most of his countrymen hesitated, was natural, since the Italian museums were filled with fossil shells, belonging to species of which a great portion did actually exist in the Mediterranean, whereas the English collectors could obtain no recent species from their own strata.

(Lyell 1830, vol. 1:31)

Though Lyell was correct that the geology of Montpellier probably influenced Lister’s ideas, he somewhat overstated Lister’s argument, confused by the conchologist’s acknowledgement of opposing views. Lister was a free and independent gentleman of means, and as Steven Shapin has shown, gentlemen-scholars would often couch their opinions in neutral language, maintaining a “disengaged posture” to infer that they were not dogmatists or “vulgar scholars” who published as their trade. Instead

the author of experimental narrative constituted himself as an antiauthor, playing upon, and negating, the egoistical and fame-seeking manner ascribed to the dominant tribe of philosopher authors. The antiauthor secured credibility by confessing his own faults ... by giving readers and authors (inadequate) grounds freely to without their assent.

(Shapin 1994:223)

Lister's remarks were thus part of a rhetorical strategy to gain credibility for his view that fossils were formed stones. Lyell also did not take into consideration that Steno's publication of the *Prodromus* prompted Lister to consider and ultimately support other more chemical models of fossilisation.

Birch's *History of the Royal Society* made note of:

A letter of Mr. Lister to Mr. Oldenburg dated at York 25 Aug. 1671 ... adding some notes on ... MR. STENO'S *Prodromus* concerning petrified shells. This letter gave occasion to some of the members to discourse on the subject of petrified shells, some applauding Mr. Lister's notions of it; but Mr. Hooke endeavouring to maintain his own opinion, that all those shells are the exuviae of animals.

(Birch 1756–7, vol. 4: 487)

Birch's summary concealed Lister's full argument. Lister not only called into question the Dane's assertion that fossils were purely organic in origin, but his response to Steno took into account *chymical* processes and morphology, as well as geology, his views proceeding as he said "not from a spirit of contradiction, but from a different view of Nature" (Lister 1671: 2282). Lister's careful study of English molluscs demonstrated to him that fossil composition was the same in which the stone in which they were embedded, and thus they were stones spontaneously created by nature alone from mineral solutions (Lister 1671: 2283–2284).

Lister then went on to write another paper on sea lily fossils or crinoids, whose form of stalk and radix appeared to him "plant-like", but most likely mineral in origin (Lister 1673: 6181–91) (see Figure 1). Lister learned of the crinoids from his mentor John Ray, who, as part of his frequent "simpling" expeditions to gather specimens for his *English Catalogue of Plants*, had trekked to remote Lindisfarne, or Holy Island of northeastern England in 1671 and 1673 (Lankester 1846: 150). In the seventh century, King Oswald of Northumbria directed the Benedictines to build a monastery there where they created the famous Lindisfarne Gospels, their illuminated pages of Celtic knots and iconic saints decorated with gold and crimson. Lindisfarne was also famous for its associations with St. Cuthbert. Originally a shepherd boy, Cuthbert became bishop of Lindisfarne, where, during his life, he was renowned for his holiness and miracles. Ray gathered on the "sea-shore under the town, those stones which they call St. Cuthbert's beads" (Lankester 1846: 150–151). The beads, which ranged in size from a pea to a half-dollar, were the ridged and perforated fossil discs of crinoids or sea lilies. Ray probably found them on the northeast part of the island among the limestone quarries mined for building material or lime plaster. The quarries dated back to the fourteenth century, and the very base of the island is part of the Carboniferous Middle Limestone Group formed between 363 and 325 million years ago (Lane and Ausich 2001: 69).

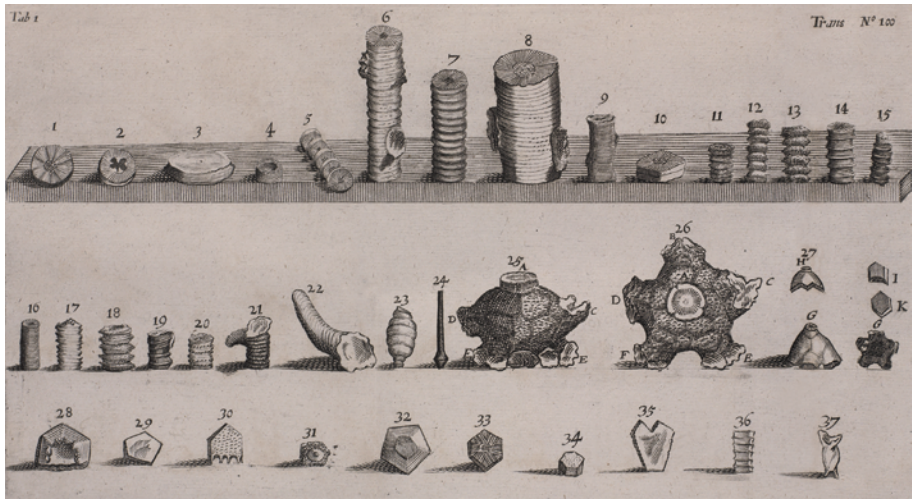


Figure 1. Illustration of crinoid fossils from Martin Lister, “A Description of Certain Stones Figured Like Plants, and by Some Observing Men Esteemed to be Plants Petrified” *Philosophical Transactions*, no. 7,100 (1673): 6191. This was the first illustrated paper on fossils in the *Philosophical Transactions*. 32 and 33 are star-shaped pentacrinoids. Courtesy of the Royal Society Library, London.

The ancient sea bed had been covered with the lilies also known as “feather stars”, echinoderms related to starfish and sea urchins. The lilies’ bases stuck to the sea floor from which grew a flexible stem of flat plates supporting a head or calyx. From the head grew five branched and moveable arms that filtered food from the sea water. Cilia in the inside of the arms manipulated the food along the arms down to the mouth situated at the tegmen at the arms’ base. The flexible branches and stem were comprised of a series of flat plates or wafers piled on each other, which were strung together by ligaments and a central coelom with a nerve cord passing through the entire sea lily. When the crinoids died, their remains accumulated to create a calcium layer cemented together by a carbonate mud that, under heat and pressure, formed the limestone. Shells and other hard parts of corals and brachiopods also comprise the limestone beds of Holy Island.

Crinoid palaeontologists N. Gary Lan and William Ausich (2001:66) have noted that, when fossilised, the plates or wafers of the stem and arms usually separate. A central hole or stem lumen remains where the nerve once was, making the “bead”. Local legend claimed that Cuthbert had made the beads to give to his brethren to string together for their rosaries. The sheer number of the “beads” Ray encountered on his walks around Lindisfarne meant Cuthbert had indeed been busy, his spirit reportedly making nocturnal visits to his “forge” on the island

to replenish their numbers. The shore is still found strewn with the beads after a storm, attesting to the saint's continued ghostly presence.

Bemused by the islanders' folklore, Ray wondered about the origin of the beads. He speculated that rather than being sacred jewellery, the small "beads" were "nothing else but a sort of entrochi" (Lankester 1846: 151). He had apparently read his Agricola who in his *De Natura Fossilium* (1546) stated:

Trochites is so called from a wheel, and is related to *lapis judaicus*. Nature has indeed given it the shape of a drum ... the round part is smooth, but on both ends as it were there is a module from which on all sides there extend radio to the outer edge ... the size of these *trochites* varies greatly, for the smallest is so little that the largest is ten times as big, and the largest are a digit in length by a third of a digit in thickness. (Agricola 2004: 256)

Ray subsequently speculated how these "entrochi" were formed. In a 1672 paper, Ray suggested to the Royal Society an inquiry into "the origin of those stones which we usually call petrified shells". He then contacted Lister to see what he thought about the matter. In a paper published in the *Philosophical Transactions of the Royal Society* in 1673, Lister mentioned that he went hunting for the beads in the villages of Braughton and Stock in Yorkshire, near the Craven Scar, finding them in "beds of clay" (Lister 1673:6181). The Craven Scar is an important series of geological faults that runs along the southern and western edges of the Yorkshire Dales, composed of former marine beds and coral atolls of folded and faulted carboniferous limestone. These bands of limestone alternate with layers of sandstone and shale in a repeating succession, and there is an underground network of caves in the limestone, limestone pavements, and dramatic waterfalls, an abundance of features to stimulate Lister's imagination.

Crinoid fossils are indeed in abundance in the Craven Scar. Lister was sent samples of pentacrinoids or star-stones from Bugthorpe and Leppington, villages 14 miles from York; these are abundant in the Lower Jurassic rock of the Yorkshire Wolds (Unwin 1995: 215). Most crinoid specimens just consist of the columnar stem, and, following Agricola's directions, Lister used vinegar to dissolve some of the limestone and break apart the columnar sutures to look at individual beads (Lister 1673:6181). A rare whole specimen also has a root structure or radix, and a head or calyx that has the soft parts of the animal, from which rose the crown of jointed arms. Lister quite reasonably thought the branching nature of the crinoids meant they were plant-like, though, as John C. Thackray (1975:333) has noted, he was "confusing arms with roots, calyx with radix". Early modern natural philosophers also had a tendency to make sweeping analogies based on surface observations of structure and function. Wondrous specimens brought from the New World also meant that old classification boundaries were questioned.

However, in this case, Lister did not think his finds were actually plants, but rather just shaped like them. His account of crinoids, which he called “rock plants”, was the first illustrated paper on fossils published in the *Philosophical Transactions*, and it was followed by another on star stone or astroites, another species of crinoid (pentacrinoids or *pentacrinites fossilis*). Again, Lister’s detailed examination of the morphology of the astroites, particularly their joints which he dissolved in vinegar and nitre, led him to conclude, “no vegetable, either of Land or Sea, that I know of, hath such frequent joints and short or thin internodia”, arguing they were mere formed stones created by mineral juices surrounding them (Lister “Astroites” 1673: 278). Treating them as mineral specimens, he speculated that “I doubt not, but they will readily calcine, as the Belemnites, of a very strong and white Lime” (Lister “Astroites” 1673: 275). Lister was in good company when he considered belemnites of mineral origin. Belemnites, cigar-shaped internal shells of an extinct squid, were regular, crystalline, and widely considered *lapides sui generis* until the eighteenth century when specimens with surrounding soft parts were found (Gould 2001: 218).

At this point, Lister did not specify how these mineral juices worked to create the “rock plants”, but he may have obtained some further ideas from his friend and correspondent John Webster, author of the influential *Metallographia*, (1671) who argued that “Metals do grow even like other Vegetables, ... manifest from divers examples”. Webster (1671:47) cited works by Robert Boyle and the Czech chemist Michael Sendivogius, but his largest intellectual debt was to Johannes Van Helmont. Webster (1611–1682) was a schoolteacher in Clitheroe, Lancashire who had studied chemistry with Hungarian Johannes Banfi Hunyades (1576–1646), chemist to the Earl of Pembroke (Clericuzio 2004). It is entirely possible that Webster may have encountered Lister’s great uncle Matthew while the latter was in the Countess of Pembroke’s entourage where he served as her personal physician and lover; as Matthew was responsible for Martin Lister’s education, there may have been a long established connection between Webster and Lister. In the 1670s, Lister was in regular correspondence with Webster about chymical matters, the two natural philosophers swapping mineral specimens of cawk, mica, and bismuth, as Lister was preparing works on iron ores and on the chemical composition on spa waters (Clericuzio 2004). Webster had adopted Van Helmont’s theories on the origins of minerals, asserting that minerals/metals had a “seminary principle to propagate themselves by”, and are generated in the “bowels of the earth, where they are contained in a liquid form” (Webster 1671:40; Clericuzio 2000:152). Van Helmont thought that “Water was the matrix of all other matter through the power of specific seeds implanted in it by God”, and advocated a liquid theory of metallogenesis (Debus 1980: 126). More specifically, the seeds of metals were situated in water, contained in a “certain hermaphroditical

salt of Mettals”, which was acidic, hungry, sharp and “considered like an Embryo, in order to a Metal”. Van Helmont’s identification of the metallic seed with salt, one of the Paracelsian *tria prima* (salt, sulphur, and mercury) was part of a larger trend in the 1630s where there was a “much more specific chemical definition of the seed and its activity” (Emerton 1984: 207).

Lister agreed with Webster and hence Van Helmont that minerals and hence fossils could grow into organic forms and their sources were probably saline in nature, but in a later publication on mineral waters, the *De Fontibus*, he disagreed with Van Helmont’s insistence that the seeds were in water. His stance was not unusual in the intellectual context of the seventeenth-century chemical debate about the formation of minerals. This debate involved French chemists, like Joseph Duchesne (1544–1609) and Nicaise La Febvre (1610–1669) who claimed there was a formative salt responsible for minerallogenesis. There were several contenders for this salt’s identity including nitre and *sal ammoniac*, but many chemists postulated that the vitriolic salt produced by pyrites was the true “universal salt” responsible for generating all minerals. The vitriolic liquid or spirit of vitriol (sulphuric acid) called “gur” or “bur” was thought by Johann Rudolf Glauber and other early modern mining authors to be a sign of the presence of mineral ores, “with which sulfurous exhalations were also associated” (Emerton 1984: 217). These exhalations from the centre of the earth, according to the German mineralogical theory of *Witterung*, were likewise thought to be the operative cause of the formation of minerals, including the transmutation of base metals into gold.

Lister also believed in *Witterung*, and in *De Fontibus* argued that the volatile exhalations of pyrites and its vitriol *in the air* (and to a lesser extent limestone) were important in the transformation of matter. Scratching or crushing pyrites does result in a sulphurous smell, and Lister claimed that pyrite and its vitriol gave off a “warm vapour” that was “largely sulphurous, pungent, and inflammable”, identifying vitriol as volatile (Lister 1684: 77). Thus, in his theory, Lister directly opposed Van Helmont who had a water-based model of metal formation. In his *De Fontibus*, Lister countered Van Helmont’s claims with detailed empirical evidence. He believed it was “pointless to state” that any sort of Helmontian hermaphroditical salt existed since “up to this point it has no characteristics and is not even recognisable” (Lister 1684: 58). His observations of pyrites also indicated that no vitriol was produced when pyrites were in water, but only when they were exposed to air (Lister 1684: 58).

Lister then took his findings and applied them to the formation of fossils. In an unpublished manuscript entitled “Method for the History of Iron”, Lister mentioned that pyrites were not only found near mineral baths, but often occurred in Yorkshire limestone and were mineralized with fossils (MS Lister 1). He thus believed all fossils contained iron marcasites, proven by running a magnet

over them, and that pyrites could be detected in a like manner. Van Helmont had argued that in the case of fossils, a petrifying and germinating seed in mineral waters produced shells “clothed in a crust as a result of rancid material from the depths” (Lister 1684, Book 2:76). In the same way, Helmont claimed “standing pooles of water do thus incrust shell fishes, which co-touch with the bottome” (Van Helmont 1664: 833). Lister again denied Van Helmont’s belief about matter formation from water, instead claiming that the origin of fossils is propagated

not so much by sexual union [of the seeds] as by the actual rancidity from the depths ... The layers of rock are produced under the earth from rancid stink arising from the rocks. I am not here enquiring how layers of rock are produced, but if we suppose that they have existed from the creation of the world the question is posed as to the source of the petrification of plants, animals and now even the rocks themselves. I declare that we are generally deceived by what has a similar appearance to certain germinating rocks, as though all other kinds of rocks were produced today from one and the same cause as was once the case. Yet as far as I have been able to observe everything that petrifies is either pyrites or some kind of limestone and nothing else. These two petrifiers produced by the continual germination of salt are after their own fashion volatile, and when admitted into intermediary substances, dead as well as alive, they combine and are compacted of their own nature, and generally even preserve the shapes of the animals, and their qualities, at least to some extent. (Lister 1684, Book 2: 76)

In other words, the “stink” coming from the rocks was the sulphurous vapour arising from the “continual germination” of salts of vitriol from the pyrites as well as calcarious nitre arising from the limestone. These volatile salts combined with intermediary substances, which could be dead plants or animals, or more usually neighbouring substances, to create “formed stones”.

In his later studies of marine bivalves and crinoids, Lister attempted to verify his hypothesis of petrification by performing microscopic and chemical analysis of the salt crystals both of fossilised shells and the shells of living molluscs (Lister 1696: 128–134). Burning the shells resulted in a calx that was identical to calcarious lime, partially bearing out his assertion that *witterung* from limestone formed calcarious fossils (Lister 1696: 128). Lister also cited Dutch microscopist Van Leewenhoek’s examination of the calx of burned mollusc shells, which when examined under the microscope, revealed six-sided salts shaped like “twigs of trees without the leaves”; the crystalline structure matching that of calcarious lime, and he confirmed these observations by repeating the Dutchman’s experiment, comparing his observations to Leewenhoek’s drawings (Lister 1696: 130). (See Figure 2).

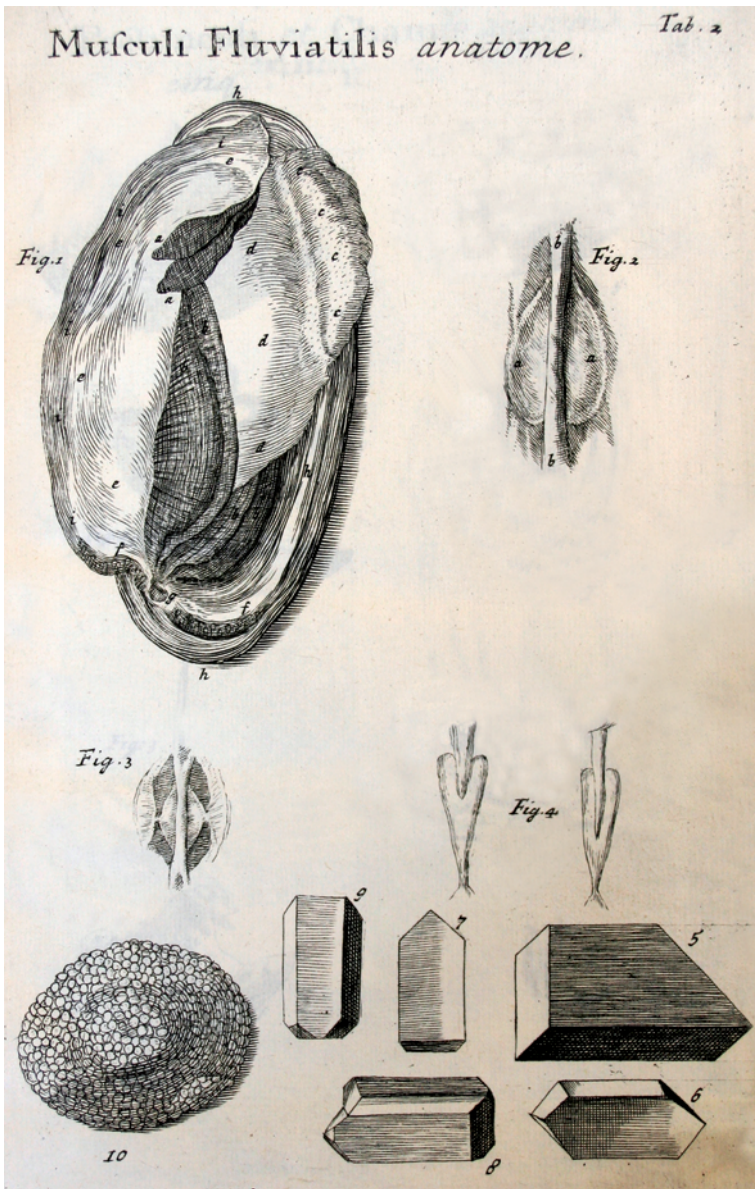


Figure 2. Lister's comparison of calcined salts from fossils and conch shells with those salts isolated by Leeuwenhoek in Figures 5–9. From Martin Lister, *Historiæ sive synopsis methodicæ Conchyliorum quorum omnium picturæ* (London: by the author, 1685–92), Table 12. The Bodleian Library, University of Oxford.

Lister's daughter Susannah had previously illustrated Leewenhoek's papers on the salt chymistry of wine and crystal growth for the *Philosophical Transactions* when Lister was vice-president of the Royal Society, so was quite familiar with the Dutch microscopist's work (Leewenhoek 1685:963–979). Inspired by Leewenhoek's study of salts, in a series of chymical experiments, Lister also attempted to prepare salt extracts from fossils and the body juices of snails, painting them over the surface of shells to see if there was an increase in weight, and tried to grow pearls from snail juices (Lister 1678:201; Carr n.d.:41). Bernard Palissy and Girolamo Cardano had also argued that shells consisted primarily of a salt extracted by the mollusc from the sea and were thus easily petrified, so Lister was attempting to recreate this process (Newman 2004:156–157). His results were inconclusive, but he clearly saw petrification and fossilisation in chymical terms.

4. Robert Plot and fossilisation

Though Lister argued against Van Helmont in his chymical model of fossilisation, his colleague and friend, Robert Plot, as a keen alchemist, was much more of a generalized advocate of Helmontian theory (Taylor 1949:67–76). Earlier treatments of Plot's view of fossilisation often mention his belief in a vague Neoplatonic *vis plastica* or “plastic virtue”, a latent character of the Earth itself or the result of some influence from the heavens. While Plot claimed that “plastick power [or virtue] or whatever else it is that effects these shapes”, and Neoplatonic arguments of this form had substantial support in the early modern scholarly community, scholars have not considered that he conceptualized this power using aspects of Helmontian salt chymistry.

As the first professor of chymistry at Oxford, Plot was certainly *au fait* with Van Helmont's work, writing a list of “chemical arcana and desiderata” which included the synthesis of several substances Van Helmont described in his chymical treatises, including Van Helmont's “Metallus Masculus” (a type of alkahest or universal dissolvent), his “Asoph”, or Azoth (the ultimate arcanum which contained the universal spirit of God in created things, its metallic signature that of liquid mercury), and his chemical arcanum against the duelech or gallstones (Gunter 1939:130–132). He proposed these topics as the subjects of discussion and demonstrations at the Oxford Philosophical Society, which, in its aims and intents, was like the nascent Royal Society. Plot's papers in manuscript also contained material concerning Van Helmont's iatrochemistry; his manuscripts included a defence of Tachenius (1610–1680), whose *Hippocrates chymicus* (first published in 1666) combined Van Helmont's iatrochemistry with Sylvius's theory of acids and alkalis in medical treatments, as well as “an example of the disease of the stone in Van Helmont” (Plot n.d. MS Sloane 3646:58–61).

It is therefore not surprising that Van Helmont's theories shaped Plot's discussions of fossils. In his chorography, the *Natural History of Oxfordshire* (1676), Plot began his discussion of the fossil debate by stating: "I am included rather to the opinion of Mr. Lister, that they are *Lapides sui generis*; than to theirs, That they are thus formed in an Animal mold" (Plot 1676: 111).

He also acknowledged in his comment on Lister that on "the shoars of the British and Mediterranean Seas, there may be all manner of Sea-shells be found promiscuously included in Rocks or Earth, and at good distances from the Sea", as well as Lister's observation that many fossilised conches were different in structure than present living creatures (Plot 1676: 119). However, unlike Lister, Plot did not think there was one chemical mechanism such as *witterung* and pyrites involved in creating the formed stones, but allowed for their formation by salt-containing mineral seeds not only in water and in the earth, but also in the air according to principles of Helmontian chymistry.

Plot wrote in his chorography of Staffordshire that

all informd Stones whatever, being upon no slight grounds, thought chiefly to have their Origin from the mixture of Salts ... the mineral Salts, I say are no less exercised in the curious formation of bodies in the bowels of the Earth and rocks, than the volatile ones in the Heavens; for it sufficeth it them not only to represent only sublunary things, but celestial ones too. (Plot 1686: 176)

Plot may have received some of his ideas from Paracelsus, who had postulated that *gamahey*, or formed stones resembling animals were formed under the influence of a certain constellation, whose virtue it bore (Paracelsus 1658: 502; Carr n.d.: 6). Plot indeed claimed that the *asteria* or crinoid fossils were formed by volatile salts shaped in the form of stars that could be impregnated with astral influences from the heavens; the fossils indeed do look star-like, each lamellar plate in a pentagonal shape (Plot 1686: 176). Though, like Lister, Plot treated these fossils as solely mineral specimens, and saw them as formed in an aeriform manner, his mechanism of celestial influence was decidedly different from Lister's argument. Astrological phenomena never entered into Lister's mind as a cause of fossilisation.

On the other hand, like Lister, Plot's conception of fossilisation was also chymical and influenced by Van Helmont. In a general sense, Van Helmont claimed that every mineral or stone:

(to wit, the Rockie Stone, the small Stone, the Gemme or precious Stone, the Flint, the Sand, the Fire-stone, the white Clay, the Earth, cocted or boyled Stones, Glasse, Lime, Sulphur or Brimstone, &c.) is changed into an actual Salt, equal in weight to its own body, from whence it was made.

(Van Helmont 1662, Chapter 8: 104)

More specifically, Plot's reference to the formative power of volatile salts, such as nitre or sal ammoniac (ammonium chloride) that gave off an aeriform component (such as an odour) and decomposed easily on heating was a mainstay in Helmontian chemistry. Van Helmont postulated a role for volatile salts in the atmosphere and in respiratory physiology that drew upon older conceptions of salts as vital, as well as Paracelsian concepts of the aerial nitre, but which had entirely new implications for iatrochemistry and medicine. In his analysis of the chymistry of the blood, he claimed that venous blood, having given nourishment to the organs of the body, was made volatile and converted into gas containing volatile salts which was breathed out (Pagel 1982: 88–90). This conversion of the venous blood into breath was thus done chemically, and was concerned with the production of fixed and volatile salts.

Influenced by Van Helmont's belief that the breath of man and animals contained volatile salts, Boyle wrote in his *Suspensions about some Hidden Qualities of the Air* (1674) that atmospheric air was likewise impregnated with such salts, and that volatile salts could draw water. Daniel Coxe (1640–1790), a London physician and Royal Society Fellow, wrote several articles in the *Philosophical Transactions* about atmospheric volatile salts that were inspired by Boyle's ideas. Coxe claimed: "... the Air, which is as I could fully demonstrate, [is] impregnated with a Volatil Salt ... partly expired from Animals during their life" (Coxe 1674: 172). Just as Van Helmont thought that heated blood discharged its volatile salts into the air, Coxe also believed that the release of volatile salt in the air was due to "Subterraneous ... and Coelestial Fires" and that these Salts, "being received into the vast subtile fluid Expanse ... becomes [sic] the Instrument of sundry remarkable effects and operations, not only in Natural, but also Artificial productions" (Coxe 1674: 172).

Many of these remarkable effects that salt produced in the atmosphere were relevant to fields of natural history, as well as meteorology, both predominant fields of study for the early Royal Society. Some of their ideas may have been suggested by Paracelsus who "repeatedly explained thunder and lightening in terms of an aerial nitre" and who also claimed that as "snow grows in the heavens, so salniter and other things grow out of the fire"; Glauber, Duchesne, and John Mayow also expressed such ideas, and we saw Lister believed vitriolic salts had the same effect (Debus 1964: 46). In his *Natural History of Oxfordshire*, Plot also argued that volatile salts were responsible for the "pleasant figurations they make in the snow" or snowflakes (Plot 1686: 176).

Plot's colleague, the botanist Nehemiah Grew, had also claimed that the short slender cylinders of snowflakes resembled those of nitre, "as also all Lixivial Salts for the most part; resembling though not perfectly, the several points of each

Starry Icicle of Snow" (Grew 1673:5195). Grew argued that every drop of rain had spiritous particles which combine with aerial salts; the regular shape of the flake was due to the energy of spirit of the rain which acted as the "pencil", while the saline parts acted as the "ruler", figuring it "into a little Star" (Grew 1673:5196).

Plot simply used these saline theories as part of his theory of fossilisation. He claimed crystalline formations could create "formed stones", the saline principle giving "bodies their figure, as well as solidity and duration" (Plot 1676: 122). The salts not only could be in the atmosphere, but inherent in seeds in the water and the earth, naturally "shooting into figures, each peculiar to their own kind; nitre always shoots into Pyramids, salts Marine into Cubes, Alum into octo, and Sal Armoniac into Hexadrums" (Plot 1676: 122). The flat spirals of ammonites could be created, for example, but "two salts shooting different ways, which by thwarting one another make a helical figure, just as two opposite winds or waters make a *Turbo*" (Plot 1676: 122). Conch fossils, on the other hand, owed their origin to "urinous salts, which shoot likewise from a centre (as suppose from the hinges of these stones) but generally are most extended to one side, as may be seen in the branched figure formed on the surface of urine by freezing"; this was depicted by Robert Hooke in his *Micrographia*. Like Lister, Plot also made use of microscopic evidence in his crystallographic analysis.

Plot then noted that *asteria* in the shape of five-pointed stars, had regular angles of 72 degrees, perhaps created by an "antimonial salt" that may "determine Bodies to this starry figure, as no question it do's in the Regulus and in the Caput Mortuum [distillation dregs] of the Cinnabar of Antimony. To such a salt may also be referr'd our Brontiae or Ombriae, and all the Echnites, [sea urchins] some whereof are plainly, all in some measure stellated at the top" (Plot 1676: 122–123). Here Plot was referring to the stellate Regulus of Antimony, well known to chymists like Van Helmont interested in the transmutation of matter (Newman and Principe 2002: 102, 108). The regulus was metallic antimony that has been reduced from stibnite with iron and nitre (saltpetre) or tartar and allowed to cool slowly under a thick slag or scoria to give a crystalline star pattern (Newman 1994: 129). The use of iron in the reduction process produces iron sulphide, leaving the antimony free as metal to sink to the bottom: $\text{Sb}_2\text{S}_3 + 3\text{Fe} \rightarrow 2\text{Sb} + 3\text{FeS}$.

Generally, the antimony would need additional purification to produce a clear stellar pattern, and different metallic reducing agents could also be introduced to separate the regulus from stibnite, such as copper, tin, lead, or iron (the regulus of Venus, Jupiter, Saturn, and Mars, respectively). There was also thought to be a mystical connection between the star like pattern of the ores and the planets, an adaptation again of the Paracelsian *gamahey*. As Nicolas Lémery's *Course of Chymistry* indicated:

the star which appears upon the Martial [Iron] Regulus of Antimony, when it is well purified, has given occasion to the chymists to reason upon the matter ... that this same star proceeded from the impression wick certain little bodies flowing from the planet Mars do bestow upon antimony, for sake of the remaining iron that was mixed with it. (Lemery 1720: 181–182)

Though Lémery was critiquing this belief, it was a common one in early modern chymistry. The term “regulus” for metallic antimony derived from the Latin “little king”; as the regulus of antimony combines easily with gold, the king of metals, antimony was important to the refining of the metal and was of significant interest to seventeenth-century chymists like Van Helmont. Like gold, antimony could only be dissolved by *aqua regia*, the king of acids (a mixture of concentrated nitric and hydrochloric acids), and this suggested some affinity between the metals as well (Emsley 2006: 21). It was thought, quite wrongly, that the “seed” of the metal used to reduce the regulus from the ore remained embedded in the regulus itself; the seed of course is in the scoria or slag and also in minute quantity in the metals applied in the work” (House 2009: 1). The source of this transmutative “seed” and its cultivation were key to the transformation of matter and the creation of the philosopher’s stone. Van Helmont also alluded in a terse and secret manner to a “higher arcanum” the “cinnabar of antimony” (Newman and Principe 2002: 104). This cinnabar was used to isolate the “sulphur of antimony”, which was believed to be a building block of metals, a transformative element, and a potent medical substance. In Plot’s case the formative power of stellate antimony salt or its cinnabar from the regulus were thought to create similarly star-shaped fossils.

5. Conclusion

Though most commentators have concentrated on the religious implications of the early modern fossil debate, the mechanisms of saline chymistry played an important role. The formative power of salts was a predominant theory in metallogenesis, and it was also used as a means to explain the creation of formed stones, whether via their volatile exhalations, or their “seeds” in waters and earths. As both Lister and Plot thought fossils were *lapides sui generis*, they have been categorized by past scholars as being upon the same side of the fossil debate, and the more subtle differences in their opinions about Van Helmont’s salt chymistry have gone unnoticed. In fact, the truth of the controversy and the chymistry inherent in the fossil debate was more complicated and carried broader implications.

Lister and Plot, along with their contemporaries in the Royal Society, such as Daniel Coxe, Nehemiah Grew, Edward Lhwyd, and John Webster were part of a largely overlooked 'salt school' significant to our understanding of the history of early modern chymistry and natural history (Roos 2007: Chapter 3). These thinkers argued that the possibility of transmutation and generation of matter was to be found in salts, particularly those that were volatile and chemically reactive, like nitre, sal ammoniac, or vitriol. Many of these late seventeenth-century English virtuosi were not physicists or strictly chymists but natural historians and physicians especially attentive to the empirical details of biological structure and anatomy. These natural philosophers argued that the crystalline regularity of salts indicated that they were basic principles of matter which were capable of the spontaneous generation of biological forms as well as substances like fossils that mimicked living creatures. They also combined their interests with the early Royal Society's research in crystallography, microscopic observation, as well as chymistry.

It is not surprising that salts of different shapes and forms were thus perceived as building blocks of living creatures, and of fossils that looked like living creatures such as crinoids. As the living counterparts of crinoids were inaccessible in the deep-sea bed, an argument that they were *lapides sui generis* was a potent one. The crinoids' unusual shapes that were not quite animal, not quite plant, also made them a focus of the fossil debates of the virtuosi; taxonomic classification of increasing amounts of novel species from the New World was a pressing concern for early modern natural historians (Ogilvie 2006: 53). Though the debate over the chemical origin of crinoids and other fossils was a specific one, it was involved in the nascent development of zoology, botany, geology, and mineralogy which were not thus mere supplements to the work in physics, astronomy and chymistry of the 'scientific revolution', but at its very centre.

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Were the arguments of William Harvey convincing to his contemporaries?

Adelino Cattani

1. Introduction

The work of William Harvey (1578–1657), who discovered the circulation of the blood (certainly one of the principal controversies in the seventeenth century), has long been considered a model of accurate observation and careful experimentation: a perfect example of the mathematical-experimental method applied to physiology. In contrast, pre-Harveyan Galenic medicine is considered a medicine of qualities (heat, cold, dry, and humid) or the so-called internal humors, such as blood, phlegm, yellow bile, black bile, and spirits.¹ The seventeenth century is considered an age in which scientists extended the experimental method and integrated physical disciplines into medicine (iatrophysics, iatromechanics, and iatrochemistry). My aim is to reconstruct how Harvey discovered and invented, then justified and supported, and finally imposed his theory within an exceptionally short time and in spite of very strong opposition.

According to conventional historiography “Harvey had demonstrated conclusively that the blood did circulate” by applying the quantitative method to the theory of the formation and movement of blood (Singer 1956:69). There are two main traits traditionally ascribed to Harvey’s revolution: the firm establishment of experiment and careful observation as the means of founding knowledge in the life sciences, and the introduction of quantitative reasoning as the basis for interpreting living processes (Cohen 1985:189). For example, Harvey provided an estimate of how much blood emanated from a contracting heart and discovered that this quantity was much greater than the quantity of food that could be consumed by the given body.

1. See, for example, Singer (1956: 15–16, 33, 63) and Temkin (1974: *passim*).

Harvey revealed this discovery in his *Exercitatio Anatomica De Motu Cordis et Sanguinis in Animalibus* (*Anatomical Exercise on the Motion of the Heart and Blood in Animals*).² The *De Motu Cordis* (DMC) is considered as a strong argument consisting of visual and logical proofs, to which confirmations constituted by “probable reasons” were added. What are the bases of Harvey’s argument? How was it justified before its opponents? Harvey provided three criteria: “careful dissections, many experiences, and accurate observations”, which in his opinion were “clear, true, established, and appropriate [means] to confer a sufficient credibility.” (DMC XIII: 65).³

Harvey studied and graduated at Padua University, whose ancient motto is *Universa Universis Patavina Libertas*, namely “total freedom for all people in Padua”. I shall exercise this freedom in considering whether Harvey’s arguments were really convincing to his contemporaries.

A careful and accurate analysis of the genesis, confirmation, and diffusion of Harveyan theory reveals that the reason for concluding that the blood in the animal body circulates continuously is a logical necessity, not a mere observational evidence. Harvey’s proof was obtained neither by experiment nor by deduction; it was not acceptable to the totality of the scientific community of his time, but only to those who agreed with his presuppositions. Some of these presuppositions were only conjectures. Therefore, Harvey’s proof has to be considered as an argumentative evidence (a proof in the sense of *pistis*) rather than a demonstrative one (a proof in the sense of *apodeixis*). It is typical of an argumentation to be conceived for and accepted by a particular audience, while a demonstration is valid always and for all reasonable people.

In common language, argumentation and demonstration are used sometimes as synonymous and sometimes as opposing procedures. The antagonistic conception of demonstration vs. argumentation represents a traditional and cultural opposition, a received view that distinguishes a family of ideas like truth, science,

2. “Exercise,” the English translation offered by Joffrey Keynes (Harvey 1995), does not correspond to the meaning of the Latin expression *Exercitatio*. Robert Willis (1847) chooses a better rendering, *Anatomical Disquisition*, which is closer to the sense of “formal academic argument.”

3. The original Latin text says: “tria illa proposita fundamenta, pro circuitu sanguinis fore aperta, vera, stabilia, ad fidem sufficienter faciendam estimamus.” All the quotations from Harvey are, unless otherwise indicated, from *The Works of William Harvey, M.D. Translated from the Latin by Robert Willis*, London, The Sydenham Society, 1847.

certainty, and reality from falsehood, opinion, plausibility, and appearance. Schematically, this is the polarity inspired by the old idea that science is grounded in demonstrative evidence while other forms of knowledge are based on argumentative processes at best.

Today, this is a very controversial distinction. Demonstration and argumentation share two features: their inferential nature and their function to prove. But they have important distinguishing traits: different subject matters, levels of consistency, addressees, grounding principles, contexts of use, and standards of evaluation (Cattani 1994: 29–32). We can say that this distinction echoes the classical, unresolved opposition between 'to be true' and 'to persuade or to convince someone of the truth of something.' Pythagoras, who showed that the square of the hypotenuse of a right angle triangle is equal to the sum of the squares of the two remaining sides, was committed to *demonstration*. Darwin, who was arguing on the grounds of a possibility, resorted to *argumentation*. In the present essay, argumentation and demonstration are used as distinct notions – neither as opposed nor as interchangeable, but as complementary means in the establishment of proof and in scientific proof.

The dialectical clash between argument and counter-argument, and the polemical struggle between opponents are particularly pronounced and evident within the climate of a scientific revolution. Once the revolution is concluded, there is a natural sympathy towards the winner and an adverse disposition towards the loser. During the revolution, however, the balance is much finer. To evaluate from a contemporary perspective the sustainability of the arguments that have historically failed and were dismissed (such as the anti-Copernican, anti-Harveyan, or anti-Darwinian ones), in the light of the subsequent developments, is not the same as evaluating them in the context in which they were proposed. In the time when they were promoted and advocated, the opponents' reasoning and evidence were not so simplistic, inconsistent, or unacceptable as they might appear today.

Harvey assumed that his data and arguments had "sufficient credibility" (*ad fidem sufficienter faciendam estimamus*, DMC XIII: 65): that they were persuasive but not decisively compelling from a demonstrative point of view. The data was argumentatively sufficient but not demonstratively complete, which explains why Harvey's concluding tone in DMC is resolute yet cautious. The epilogue of DMC, the last words of this revolutionary treatise (XVII: 87), refer more to a dialectical confutation than to an experimental one. It is a strong claim about the impossibility of the contrary:

All these appearances, and many others, to be noted in the course of dissection, *if rightly weighted*, seem clearly to illustrate and fully to confirm the truth contended for throughout these pages, and at the same time to stand in opposition to the vulgar opinion; for it would be *very difficult to explain in any other way* to what purpose all is constructed and arranged as we have seen it to be.

(DMC XVII: 87; italics added)⁴

These assertions rely more on the indefensibility of the opponents' opinions than on the truth of the proponent.

In Harvey's opinion, it would have been very difficult for anyone to explain by any other means than the ones he had used, what causes our body and all the things described to have such a structure. Namely, he realized that his was only a plausible explication, a highly probable one, but that there were other possible explanations; he challenged the opponents to give a better explanation, given the body of conformation that he had conceived and presented (DMC XVII: 87).

2. Harvey: Between observation and reasoning

My central question here concerns the foundation of Harvey's demonstration of blood circulation. A related problem is how and when a scientific controversy is to be considered as settled. I will attempt to show that Harvey defended his new system of blood circulation against his detractors not only with "ocular demonstrations" and "reasoned arguments" (DMC XVII: 58), but also with acumen and brilliant argumentative tools, or, in other words, with mental experiments and persuasive arguments.

Persuasive arguments are not necessarily convincing arguments, because persuading and convincing are two different acts. In western languages, the difference between the two terms seems clear, and it is a distinction developed theoretically by many authors. Kant and Perelman agree on this.⁵ Persuasion is centered chiefly on the speaker, and it stresses one's will and ability to modify

4. "Haec omnia phaenomena inter dissecandum observanda, & plurima alia, si recte perpensa fuerint, ante dictam veritatem, videntur luculenter illustrare & plane confirmare, simulque vulgaribus opinionibus adversari: cum quam ob causam ita constituta sint, & facta haec omnia difficile cuiquam admodum sit nisi quo nos modo esplicare" (DMC XVII: 87).

5. The distinction between two kinds of belief is present in Kant's vocabulary: *Überzeugung* (conviction, more objective) and *Überredung* (persuasion, more subjective). Cf. Kant 1787 Part II, Chapter II.3. Cf. Kant 1799 §53, where *Überredung* is connected with *Überlistung* (deception). Cf. Perelman and Olbrechts-Tyteca 1958: §6.

other peoples' opinions and behavior; convincing is centered chiefly on the addressee, focusing on other peoples' capacity to be convinced and evaluate rationally. By considering the two English verbs 'to convince' and 'to persuade' (or the corresponding French, Italian, or Spanish expressions), we easily realize that they are not synonymous. For example, unlike 'to persuade,' the verb 'to convince' is synonymous with 'to demonstrate,' 'to prove,' 'to verify,' and 'to induce' someone to do or think something by means of verification. This elementary linguistic test suggests that 'to persuade' should be basically connected to the idea of a process, while 'to convince' should be linked to the idea of a product. A process is dynamic while a product is an end result. What is interesting in a process is not only the final verdict or result, but the way in which it is reached. Similarly, what is interesting in a trial or even in a scientific controversy is not only the end result, but also the way the participants conclude or arrange it.

Harvey's proof is very persuasive, but it only demonstrates the relative superiority of a thesis when it is contrasted with another thesis. Defending the traditional Galenic explanations, many of Harvey's opponents were in fact more reasonable than commonly assumed by historians of science and epistemologists.⁶ The central dilemma seems to revolve around the question whether "there is remarkably little that one can point to, even 375 years later, that is factually 'wrong'" in *DMC*, because Harvey's "logic aimed to be rigorous and convincing, and proved so, demonstrating his theories by direct observation and innovative experiments!," according to Frank (2005: 6–7), or whether it contains incredibly many erroneous observations and conclusions, as claimed by Kilgour (1954: 411; 1957: 62).

The intellectual process that led Harvey to identify a different path for the movement of the blood is a matter of a controversial historical debate. Pagel (1966) and Whitteridge (1971) advance two different views regarding the genesis and the justification of Harvey's theory: according to the linear-observational interpretation supported by Whitteridge, only the facts (the sound and extensive observational scaffold) were determinant; according to the discontinuous-analogical interpretation, offered by Pagel, the cues and the presuppositions were decisive – the facts were inferred and deduced.

Does the pathway pointed out by Harvey correspond to the pathway he had actually followed in his discovery, or is it a rationalization of it? In the first lines of the *Introduction*, Harvey addressed Dr. Argent, President of the Royal College of Physicians:

6. Including some outstanding scholars such as Whitteridge (1971). See also Flourens 1857: 51–52.

I have already and repeatedly presented you ... with my new views of the motion and function of the heart, in my anatomical lectures; but having now for nine years and more *confirmed these views by multiplied demonstrations* in your presence, *illustrated them by arguments*, and *freed them from the objections* of the most learned and skillful anatomists. (DMC Dedicatio: 5; italics added)⁷

Harvey was proud of this discovery, recognizing that it overturned a view at least as old as Galen's. He presented it to his audience in the College together with the vivisectional experiment. It was a thesis to be defended, as any disputation in a university. This disputation was presided over by the President of the College, just as there was an arbiter in most university disputations. Harvey's colleagues did not universally accept his thesis, as indicated by the objections that had been raised (French 2002: 11).

Harvey's genius was that he was able to deliver his *demonstratio* largely and decisively by way of inferential reasoning, not by direct and accurate observation. It is certainly an exceptional argument, well developed, and a strong example of how one can construct "a maximally persuasive argument", as Frank contends (2005: 7). It is a persuasive one, but not a compelling one, because the "experimental inquiry", "the close observation", and "the direct experimentation" are open to refutation and dispute. We have to consider that until the late sixteenth century, "the basic premises for syllogisms were ideally Authority (the ancient authors and the church)", and only subsequently, "experience of sensory observation could also be included", when what had previously been considered as disputations in medieval universities, were becoming "controversies" in the medical faculties (French 2002: 9).

How did Harvey's theory of the circulation of the blood – upsetting presuppositions and explanations normally accepted in the physiological field – succeed in imposing itself in a relatively short time? What is the foundation of Harvey's demonstration of blood circulation? How did he justify it in the face of his opponents? I will address four issues about Harvey and his discovery about the circulation of blood: its discovery and invention, its justification and demonstration, its consolidation and acceptance, and finally, the demonstrative or the argumentative nature of his proof.

7. "Sed iam per novem & amplius annos multis ocularibus demonstrationibus in conspectu vestro confirmatam, rationibus & argumentis illustratam, & ab obiectionibus doctissimorum & peritissimorum Anatomicorum liberatam" (DMC Dedicatio: 5).

3. Blood circulation: Discovery and invention

The way Harvey reached his idea is slightly different from how it is traditionally narrated and represented. "The heart of creatures is the foundation of life, the Prince of all, the Sun of their Microcosm" are the opening words in the *DMC*, which is dedicated "to the Most Illustrious and Indomitable Prince Charles, King of Great Britain, France, and Ireland, Defender of the Faith" (*DMC* Dedicatio: 3).

This might seem to be a mere analogy and homage to a patron, but the correspondence between macrocosm and microcosm, between the planetary and organic systems, is repeated and refined in the central Chapter 8 of the treatise on the heart, where we find an opposite comparison: "The heart is ... the sun of the Microcosm, even as the sun in his turn might well be designated the heart of the world" (*DMC* VIII:47). This assertion is intriguing because the connection between microcosm and macrocosm carried prominent alchemical and astrological overtones.

One of the relevant points of discussion, and one of the causes for the hostility towards the Harveyan thesis was certainly the issue of the heart's centrality. The objects of debate were both the physical-topological centrality of the heart (its location) and its importance (its centrality in the sense of function and finality). Likewise, the location and the respective importance of the sun and the earth were problematic. These considerations notwithstanding, what made Harvey introduce novelties about the motion of the heart is more remarkable: he tried to be innovative since he was unsatisfied with the current *opiniones* and *placita*. That is, the explanations concerning the motion and the function of the heart and arteries appeared to him to be "obscure, or inconsistent or even impossible" (*DMC* Introduction: 19). It was this very uneasiness that inspired him to look for a better explanation, which he admits explicitly in the pivotal Chapter 8:

I began to think whether there might not be a *motion, as it were, in a circle*. Now this I afterwards found to be true ... Which motion we may be allowed to call circular, in the same way as Aristotle says that the air and the rain emulate the circular motion of the superior bodies; for the moist earth, warmed by the sun, evaporates; the vapours drawn upwards are condensed, and descending in the form of rain, moisten the earth again; and by this arrangement are generations of living things produced; and in like manner too are tempests and meteors engendered by the circular motion, and by the approach and recession of the sun.
(*DMC* VIII: 46)

We can observe that what Harvey proposes is more akin to a cycle than to a real circular motion, which suggest that, at least originally, Harvey had a concept of circularity comparable to a process of evaporation or sublimation. His

observations refer explicitly to the cycle of certain natural phenomena, like rain, the seasons, or generations, and not to the circle, as in the circular motions of the heavens.

Another quotation may illustrate Harvey's approach. First, he wrote at the end of the Introduction to his *De Generatione Animalium* that, above all others, he followed Aristotle among the Ancients, and Hieronymous Fabricius ab Aquapendente among the Moderns; he followed the former as his leader and the latter as his guide (Harvey 1847: 167). Harvey is rightly considered a scientist who overthrew a scientific explanation that had endured for ca. 1550 years and who introduced quantification, calculation, and experimental observation into physiology. The Aristotelian nature of his world-view needs therefore some justification. "It is very strange that in the midst of this, his great discovery, he [Harvey] should return again to an Aristotelian position. But so it is!" (Singer 1956: 56). Even if Harvey's theory of circulation restored the ancient Aristotelian theory of the centrality of the heart – not because he was fascinated by it but because challenging Aristotelianism was difficult, even in the seventeenth century – we have to say that the Aristotelian notion of circularity, the idea of the correspondence between the microcosm and macrocosm, and the heart seen as the "Sun of the microcosm", testifies to the peculiarity of Harvey's reasoning. Accuracy and intuition, experiment and speculation, and the conventional and novel views were combined fruitfully and led to a revolution.

Not all fallacies, such as "the fallacy and the danger of hypothetical or theoretical thinking" or "the fallacy of testimony [and], the deference to authority",⁸ have harmful effects on the improvement of science: in Harvey's case, these two fallacies comprised the source of a lucky and extraordinary discovery.

8. Gilbert Blane, a fellow of the Royal Society, in his handbook of "medical logic illustrated," elucidates the various "principles of truth and errors in the cultivation of medicine" and enumerates the sources which have most materially obstructed the improvement of medicine – namely: (1) the fallacy and the danger of hypothetical or theoretical thinking; (2) the diversity of constitutions; (3) the difficulty of appreciating the efforts of nature and of discriminating them from the operations of art; (4) superstition; (5) the ambiguity of language; and (6) the fallacy of testimony – with some remarks on the excessive deference to authority and fashion (Blane 1822: 156–157).

4. Blood circulation: Justification and demonstration

What is important, however, is not how an idea comes to a scientist, but how he proves or verifies his assertions. Obviously, the former considerations concern the historical-psychological phase of discovery. We now have to look beyond the genesis of the notion: we have to examine how the new theory was justified from the observational and experimental point of view, and how it subsequently imposed itself.

At least four problems remain unresolved in Harvey's theory. First, we find incomprehensibly erroneous calculations and quantifications regarding the frequency of pulsation and the blood's flow. Harvey estimated a heart stroke volume that is only a twentieth of the minimum value (he calculated at least 3.9 grams "transfused through the heart in the arteries" at every beat instead of about 89 grams as is now known). He calculated a heart rate that is half the real one and half its minimum value (he estimated from 33 to 133 pulses per minute). Further, he considered a rate value of the blood that is about one thirty-sixth of what is now known to be the case (*DMC IX*: 43). These erroneous figures are pointed out and discussed by Kilgour (1957:62) and by Alessio (1963:752). Such errors and underestimations may be irrelevant, as some historians suggest: Harvey may have chosen the lowest possible figures in order to demonstrate the absurdity of the claim that blood was produced in the liver (the source of the blood in Galen's theory) and the impossibility that the tissues could consume such a quantity of blood produced over a day. However, we must consequently say that what is revolutionary about his account is not the actual measurement and weighing, but rather the very idea of computation being applied to life processes.⁹

Secondly, Harvey tried to explain the mechanism of circulation, not its function or aim (the final cause). In his time, not being able to explain "why" something is the way it is constituted a serious problem. For his contemporaries, the flaw in Harvey's reasoning was that he was not able to explain why circulation took place, even if he was able to prove how it happened. It is worth noting that contemporary anatomists held that the first two reasons for elaborating an anatomical theory, and, *a fortiori*, an anatomical change, "were philosophical and religious, that is, to place anatomy in a scheme of learning as a whole and to explain how God had put the body together in a rational and wise way" (French 2002: 8).

9. Harvey was not the first inquirer to apply a quantitative method to the study of physiology. Early in the seventeenth century, Santorio Santorio, professor of theoretical medicine at Padua, designed a thermometer and other instruments, such as a weighing chair, to check the human body.

After all, even today a science that deals only with how natural phenomena occur without saying much about their purpose is considered imperfect. The third unresolved problem was not having understood the function of the so-called “little circulation” (the pulmonary circulation and the breathing function).

Finally, and most importantly, Harvey did not demonstrate, and did not observe, any connection between veins and arteries, a gap in his theory that he deplored. Indeed, he writes, undecided, “the blood passes from arteries into the veins either by means of anastomosis or indirectly through the porosities of the flesh, or by both means” (*DMC* XI:57). More significantly, in *The First Anatomical Disquisition on the Circulation of the Blood*, addressed to Jean Riolan, in 1649, twenty-one years after the publication of *DMC*, Harvey (1847: 103) admitted:

I have myself pursued this subject of anastomosis with all the diligence I could command, and have devoted considerable time and labor to the inquiry; but I have never succeeded in tracing any connection between arteries and veins by a direct anastomosis of their orifices. I would gladly learn of those who give much to Galen, how they dare swear to what they say. Neither in the liver, spleen, lungs, kidneys, nor in any other viscus, is such a thing as an anastomosis observed.¹⁰

Thus, it is not surprising that his hypothesis was deemed “paradoxical, absurd, impossible, incomprehensible, and injurious to human health” by Guy Patin; and untrue, unusable, unnecessary, and ridiculous by many other eminent scholars and contemporary scientists, such as Riolan, James Primerose, Emilius Parisanus, Willem Piso, Gaspar Hoffman, Oluff Worm (or Wormius). All of them are considered by modern historians of science as obstinate in opposing the theory of circulation, as unreasonable, or as being sarcastic (Flourens 1857: 51–52).¹¹

Harvey used both dissection and vivisection in his inquiries, but he was not able to see any conjunction between the arteries and the veins by *autopsia* (literally: “to see for oneself”). This was obviously impossible before the invention of the microscope. Today, we know that the blood stream goes thorough the capillary

10. The quotation is from Harvey’s *Exercitatio anatomica de circulatione sanguinis, ad Riolanum, Prima*, first published in 1649: “Ego qua potui diligentia perquisivi, & non parum olei & operae peridi, in anastomosi exploranda, nusquam autem invenire potui vasa invicem, arterias scilicet com venis, per orificia copulari: libenter ab aliis discerem, qui Galenum tantum adscribunt, ut ad verba eius jurari ausi sint. Neque in jecore, liene, pulmonibus, renibus, aut aliquo viscere, est aliqua anastomosis”.

11. “Je raconterai ... le ridicule entêtement que la Faculté mit à repousser la circulation, les mauvais raisonnements de Riolan, la plaisanterie inopportune de Guy-Patin” (Flourens 1857: 51–52; see also Chapter VI, *De Guy Patin et de la lutte entre l’ancienne et la nouvelle physiologie*, 177–199; and Chapter VII, *De Guy Patin et de la Faculté de Paris*, 200–221).

network, an observation that was made possible by Marcello Malpighi in 1661, four years after Harvey's death. "There was one part of circulation, however, that he left still a matter of inference. He had shown that blood passed from arteries to veins, but he had not actually seen the passage" (Singer 1956: 69). Harvey failed in explaining this gap to his sceptical contemporaries, and so he left the matter undecided and the question unresolved.¹²

Did Harvey really pass triumphantly "from observation to inference, from inference to verification in an orderly and stately sequence", as Singer suggests (1956: 67)? Was Harvey's discovery really "worked out and thought out, point after point, until all was clear as sunshine in midsummer?" (Franklin 1961: 15)¹³ Harvey advanced three propositions as basic premises for the circulation of the blood, which he believed were sufficiently established and compelling as to confer credibility. The three propositions concerned the continuity, quantity, and direction of the blood flow:

1. The quantity of blood passing through the heart and the lungs: "the blood is incessantly infused into the arteries in larger quantities than it can be supplied by the food" (*DMC* X: 52). Not only does the blood pass through the arteries continuously and in greater abundance than can be furnished by the food eaten, but also in a much larger quantity than is needed for nutrition.
2. The veins are the vessels carrying the blood from the extremities to the center of the body only.
3. The presence of the valves in the veins.¹⁴

Referring to this last point, Robert Boyle had asked Harvey about the reasons that led him to think of the circulation of the blood:

12. Cf. Alessio 1963: 749–750, n. 50.

13. The expression is referred to by G. T. Bettany in his *Eminent doctors: Their lives and their works*, 1885, I, quoted by Franklin (1961: 15).

14. In Harvey's time, students studied in Padua in the same way as today's students do in the prominent Anglo-Saxon Universities. Padua was the center of many controversies and a prestigious medical school in Europe, whose luminaries included Vesalius (1514–1564), Colombo (c. 1516–1559), Falloppio (1523–1562), and Fabricius ab Acquapendente (1553–1619). Andreas Vesalius was the author of a well-known anatomical treatise *De Humani Corpori Fabrica* and Fabricius ab Acquapendente provided the first good description of "little doors" in the veins, in his *De venarum ostiis*. In Harvey's opinion, the function of the valves in the veins was to force a unidirectional flow from the periphery of the body to the heart for the passing blood. The logical consequence of the presence of valves in the veins, in Harvey's judgment, was, therefore, that the blood is circulating.

He answered me that when he took notice that the valves in the veins of so many several parts of the body, were so placed that they gave free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way: he was invited to imagine, that so a provident a cause as Nature had not so placed so many valves without design: and no design seemed *more probable* then that, since the blood could not well, because of the interposing valves, be sent through the arteries, and return through the veins, whose valves did not oppose its course that way. (Franklin 1961: 51–52; italics added)

Probable is a key word and a revealing term: it is merely probable that this is the case. If we were to provide a formal structure to Harvey's reasoning, his inference would look as follows:

1. The circulation of the blood implies the presence of valves in the veins.
2. There are valves in the veins.
3. Therefore, the blood circulates.

In theory, the reasoning should be logically rejected; in practice, it was proposed and accepted as sound. The conclusion should actually be "it is plausible that the blood circulates" or "we have strong reasons to conclude that the blood circulates" rather than "the blood circulates". This type of reasoning is comparable to Peirce's "abduction", which is a type of inference yielding an explanatory hypothesis, rather than a deductive or an inductive conclusion. The form of abductive inference is as follows:

1. A surprising phenomenon P is observed.
2. P would be explicable as a matter of course if H were true.
3. Hence there is reason to think that H is true.

(Peirce 1931–1960, Chapter 5: § 189; Hanson 1969: 86)

Abduction is a special and significant reasoning procedure but it can hardly support a conclusion or confirm a hypothesis even if "abductive confirmation is often the *only* evidence for many central theories in science, or in any realm of investigation" (Bybee 1996: 44). In fact, this "detective reasoning" is excellent for discovering both the causes of an event and those responsible for a crime. Harvey's inference seems like an intelligent exploitation of a "non-scientific" (for the old standard view of science) way of reasoning that is different from both induction and deduction. Harvey's case testifies to the existence of forms of scientific argumentation in which fallacies, such as affirming the consequence, are not only used and practised, but are actually usable and functionally applicable.

The circulation of the blood was more constructed than discovered. The concluding words of Harvey's treatise, "All these phenomen ... if they be rightly weighted ... it were very hard for anyone to explain by any other way than I have done" (*DMC* XVII: 87) confirm his awareness of this fact.

5. Blood circulation: Fulfillment and acceptance

A scientific controversy is often considered as settled when the logical arguments and the factual evidence supporting one of the sides prevail. When the validity and consistency, quality and relevance, and quantity and clarity evince the superiority of one side, it becomes overwhelming, while the other succumbs. If a theory matches such requirements, it is plainly accepted.

At this point, *les jeux sont faits*, and we have winners and losers. The outcome is that the 'losing' side becomes overshadowed. During a scientific revolution, however, strong dialectical confrontations between arguments and counterarguments, and a polemical clash between opponents naturally occur. When the revolutionary stage is completed – when a theory 'defeats' a rival one – the winning party is subject to a favorable recognition. During a period of radical changes, however, the scales of the argument are much more balanced.

Whereas Harvey's experimental apparatus is defective, his argumentative skills are strong. It is thanks to this skill that the circulation theory arose, was justified, and became popular rather quickly, considering that it was a revolutionary theory. Harvey's most significant tool was the estimation rather than accurate calculation or direct measurement. After all, Harvey himself said: "let us suppose ... either by our thought or by experiment" ("supponamus, vel cogitatione vel experimento"; *DMC* IX: 43).

Harvey's proof is a dialectical, refutative, or an elenctic demonstration (*elenkos*), in the Aristotelian sense (Aristotle, *Metaphysics*, Book 4: 1006 a 6–9 e 15–18). That is, it is neither a deductive evidence nor an inductive one but a proof of the impossibility of the contrary. A kind of reasoning in which we can prove the principle but not the observations, reserved for the cases where the only way to resolve a controversy, for lack of evidence, is to confute the adversary's position. The very conclusion of *DMC* (XVII: 87), it is worth stressing, seems to be a real dialectical confutation: it shows clearly the inconsistency and the falsity of his opponents' opinions rather than demonstrating its own validity.

On the basis of an internal and historical reconstruction of the genesis and justification of Harvey's theory, we can affirm that in spite of the received views

and notwithstanding what Harvey said, in the discovery of blood circulation, the inventive factor was prominent, and that in his proof the argumentative component was essential. Harvey's discovery was a happy inference, supported by data and experiments that were interpretable in different ways (as is often the case). It was, therefore *experimentally demonstrated* only for a few people, not for all reasonable persons. Being convincing for some audiences (even if the majority) but not universally is the peculiar trait of a rational argumentation rather than a feature of a factual and compelling demonstration. Harvey's reasoning, like many other scientists' reasoning, if we consider its structure, reveals itself as a hybrid. This makes it difficult to maintain a clear distinction between the evidence grounded on persuasive argumentation and the evidence based on convincing demonstration.

Celebrating the fourth centenary of Harvey's graduation in Padua, Roger French wrote the following words, which capture well the essence of the issue at stake:

The story of William Harvey is one of paradoxes. He was a great admirer of the ancients, yet he made a discovery which refuted the medical system derived from them. He was quite opposed to the new philosophers, who want to oppose the systems of ancients, but who had nothing enduring with which to replace it. He could demonstrate the circulation, but he could not prove it in the traditional manner, that is to show what its purpose was. His doctrine reached more people because it became involved with the new system of Descartes, with which Harvey was not in agreement. (French 2002: 13)

6. Harvey's proof as argumentation

Harvey's proof did not consist of visual evidence. The actual "visual" demonstration was offered some decades after the publication of the *DMC* (1628) by Malpighi (1661) and van Leeuwenhoek (1688), whereas Harvey could not show the connection between arteries and veins and miscalculated the quantity of blood circulating in the human body. All this enhances Harvey's achievement: he was able to impose his theory despite the disbelief of many authoritative opponents, on the grounds of an argumentative certainty instead of empirical evidence.

Harvey's proof was not an experimental demonstration. To be sure, he affirmed that his thesis was proved "by ocular demonstration for nine and more years, freed from the objections of the most learned and skilful anatomists" (*DMC* Dedication: 5). In spite of this, and despite the traditional epistemological interpretations, Harvey did not really confirm his views by way of demonstration in the presence of other natural philosophers, but he illustrated his views by using excellent arguments. Nor did he respond to all the doubts and objections raised against him.

Did Harvey's proof comprise a demonstration? We should answer this question negatively if by demonstration we mean reaching a decisive and necessary truth. If, however, we define demonstration as a convincing or persuasive proof of an argumentative nature, we might answer positively. More specifically, it conforms to the model of abduction in Peirce's sense of the term.

In fact, *DMC* "remains a marvelous example ... of how to construct a maximally persuasive argument" (Frank 2005: 7), even if it is not the typical example of the mathematical-experimental method applied to physiology, as it has been commonly viewed. We can say that Harvey first devised and then discovered the circularity of the blood. He neither observed nor fully demonstrated it by experiment; yet once he discovered – or, more accurately, invented – this circularity, he proved its necessity. This is not surprising, since science sometimes advances by way of reasoning retroactively, by way of working from conclusion to premises. The revolutionary scientist is often like a detective or a defence lawyer: his starting point is a "truth" that he regards as certain and works his way "back" to the premises. It is by argumentation however, rather than by demonstrated truths that science makes progress, through inevitable and essentially important controversies.

My answer to the title question of this essay is that, while Harvey's arguments may have not been completely convincing to his contemporaries, they were strongly persuasive: Harvey's proof is best seen as an argumentation *ex consequentibus* rather than a compelling experimental demonstration.

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Why was there no controversy over Life in the Scientific Revolution?

Charles T. Wolfe

“Of all natural forces, vitality is the incommunicable one”
(Fitzgerald 1945: 74)

1. Introduction

To ask why there was no controversy over Life – that is, debates specifically focusing on the status of living beings, their mode of functioning, their internal mechanisms and above all their ‘uniqueness’ within the physical universe as a whole – in the Scientific Revolution is to simultaneously run the risk of extreme narrowness of detail and/or of excessive breadth in scope. That is, if we take the question ‘why?’ at face value, a succinct answer can be given right away: the Scientific Revolution is an intellectual construct that we owe chiefly to the likes of Alexandre Koyré and Herbert Butterfield, and it was almost entirely focused on the physico-mechanical sciences; the latter focus was rarely challenged in the successive historiographic reassessments of this episode which emphasized notably its Puritan, gentlemanly or courtly roots,¹ and said not a word about the life sciences.²

1. A partial exception would be Kuhn 1976 precisely because he introduces a distinction between Baconian-inductive programmes in natural philosophy and more mathematically oriented programmes. As recently as Biagioli 1998 the standard picture which disregards the life sciences is still reiterated, only now with a constructivist flourish that has accents of *épater le bourgeois*: we learn that historians of science still need the concept of the Scientific Revolution to preserve their employment (Biagioli 1998: 144); but we are able to challenge the existence of this historical episode: “If we don’t overdo it, we can safely bite the hand that feeds us” (ibid., 146).

2. Debus 1991, 2001 are exceptions, but they are precisely not ‘mainstream’ Scientific Revolution narratives. My point is not that no one has studied early modern life science (the bibliography of the present essay is filled with references to such studies) but that such studies never seem to impact on the picture we have of the Scientific Revolution. Put differently, if we consider as crucial a figure as Harvey and we look at what mainstream history of modern science has

Hence our concept of the Scientific Revolution does not include debates over generation, *semina rerum*, species, anatomy, vivisection, animal souls, irritability and so forth.³ Conversely, the title question appears enormous and unmanageable once we realize that it implies several other interrelated questions:

- (i) If Life was *not* a topic of controversy in and for the Scientific Revolution, when did it become one?

This question requires that we decide what counts as a controversy; for surely debates between Harvey and Riolan on the heart, or Whytt and Haller on irritability, do not meet the criteria for a 'strong' definition of a controversy in the history of science, i.e., in Helga Nowotny's definition, as "part of the collective production of knowledge the very lifeblood of science, one of the most productive factors in scientific development" (Mendelsohn 1987: 93; see also Dascal 1998).

- (ii) How should we then understand the various activities that existed at the time, from anatomy and physiology (or the study of the 'animal economy') to medicine overall, as well as natural history, botany, or chemistry?⁴

Notice that even when we discard monolithic concepts of the Scientific Revolution and adopt a much more nuanced approach to the disciplinary status and diversity of natural philosophy, as Domenico Bertoloni Meli (2008: 709) does in an exemplary recent article, emphasizing the interplay between the mathematical and medical disciplines, so that "when unraveling the intellectual world in the

to say about him, here is a sample result: "Shapin's *The Scientific Revolution* has two derivative remarks, Gaukroger's comprehensive treatise on western scientific culture mentions him twice in passing, but only in the context of the science that came after him, and Dear's compilation of readings from *Isis* on early modern science omits him altogether... Kuhn's celebrated analysis of radical disjuncture in the progressions of science sees Bacon, Copernicus, Lavoisier and Newton as revolutionary but not Harvey, who is excluded from his account entirely" (Salter 2010: Introduction). Smith 2009 is an interesting stock-taking of changes wrought in the Scientific Revolution narrative(s) over the past twenty years but also takes no notice of the effect a focus on the life sciences might have in this context.

3. It is a mark of the extreme provincialism of the history and philosophy of early modern science that it is so preoccupied with endless internal controversies over externalism and internalism, the role of the Church, or the replicability of an *experimentum crucis* that it takes absolutely no notice of recent (and original) work on the specifically 'biological' contribution to debates on substance, personal identity, species etc. in early modern thought, in which figures like Gassendi or Walter Charleton loom large (see notably Fisher (ed), 2003 and Smith (ed), 2006). National or linguistic provincialism (Anglocentrism) has been discussed elsewhere, notably with respect to Merton's and Shapin's theses.

4. For an early attempt to answer this sort of question see Roger 1980: 258f.

seventeenth-century, we can no longer separate the history of anatomy from the history of science as if anatomists and physicians inhabited a different world from not only mechanical and experimental philosophers, but also mathematicians,”⁵ this still does not help us answer question (ii) above. We could extend the narrative of the Scientific Revolution to include debates on the circulation of blood, or the usefulness of the microscope (say, Borelli versus Locke), or the epistemological status of the ‘animal economy’ in relation to machine as well as soul.⁶ Alternatively, we could be more faithful to the actual contents of the reports presented to the Royal Society or the *Académie des sciences* in their first fifty years of existence, which turn out to be much more biologically oriented than traditional historiography has led us to believe.⁷ But in either case, these extensions would miss the dimension of ‘crisis,’ that is, the sense that the existence of living beings suddenly again becomes an explanatory challenge or even a “scandal”, whether from the standpoint of physics or on the contrary from the standpoint of the autonomy of biology.⁸

In addition, our title question also implies a historiographic claim about the Enlightenment, which follows from (i):

5. To my knowledge the first to raise the issue was Salomon-Bayet 1978: 12, 15, 112, 334 and in the Anglophone literature Cook 1990: 401–404. To mention one more example: out of fifteen essays by prominent scholars in a recent volume on ‘Rethinking the Scientific Revolution’ (Osler [ed], 2000), not a single one treats the life sciences even secondarily. Kiernan 1968 argues for a split between physical sciences and life sciences throughout the eighteenth century in France, which is very strange, if we consider figures such as Maupertuis, who sought to extend Newtonian concepts into the realm of generation, or Buffon, who translated Stephen Hales and sought to produce a kind of ‘arithmetic’ of life. Even Diderot declared that his essays on probability were, together with the *Rêve de D’Alembert*, his favourite amongst his writings (Diderot 1961: 126).

6. The first case is now too frequently discussed for citations to be necessary; on the latter two cases see Salter and Wolfe 2009 and Wolfe and Terada 2008.

7. E.g. Hahn 1971, a classic study of the Académie des Sciences, completely omits the life sciences; Kaplan 1993 fails to consider that ‘embodied forms of knowledge’ really were an issue for the Royal Society. Conversely, in a recent attempt to produce a historical survey of the philosophy of biology (Grene and Depew 2004), the authors bypass our era and thus our controversies entirely, by moving from Aristotle to Descartes and then to Buffon. That the seventeenth and early eighteenth centuries might have expressed something of a ‘crisis’ on topics such as body, species and the minimal constituents of living matter (e.g. Gassendi, Charleton, Lamy) is not an issue for this work.

8. Thus Jacques Monod described the discovery that motivated him to go into biology: “the scandal that certain objects exist with the properties of living beings and appearing to violate some physical principles or at least the general notion of the physical world. It seemed scandalous to me. I felt naïvely that one had to confront this scandal” (Monod 1970: 50).

- (iii) Life is a controversial topic for the eighteenth century, not the seventeenth (regardless of the varied and significant contributions of Sanctorius, Harvey, Glisson, Malpighi, Baglivi, Descartes, Guillaume Lamy, Swammerdam, Van Helmont and others).

This incidentally seems to reverse Foucault's claim in *The Order of Things* that Life did not exist before the emergence of biology as a science bearing that name, in the nineteenth century,⁹ as well as the much more common claim, found typically in histories of physiology and related textbooks, that the 'modern', functionally specified concept of man as machine successfully banished concepts such as Life from science, especially once the 'machine' is augmented with Darwinian evolution and the modern synthesis.¹⁰

The irony here is that it is precisely after Cartesian or LaMettrian concepts of *bêtes-machines* or *hommes-machines* that Life becomes a locus of a kind of ontological crisis, either because natural philosophers worry about what it is, what its minimal conditions and components are, or because they worry about the boundaries between dead and living matter – such as when Buffon, in his comparison of the animal and vegetable kingdoms, ponders the exact nature of “le vivant et l'animé”: whether Life is a metaphysical property of certain entities (“un degré métaphysique des êtres”) or a “physical property of matter”; he ultimately opts for a kind of ‘panspermist’ hypothesis in which life is always potentially present in matter, notably in the form of organic molecules, so that “raw matter” merely means “dead matter”.¹¹ Similarly, Gabriel-François Venel, in his long entry “Chymie” for the *Encyclopédie*, states that organic molecules and organized bodies are

9. Foucault 1966, including the claim that “l'histoire naturelle, à l'époque classique, ne peut pas se constituer comme biologie” (173). There has been much debate about what Foucault meant here, and rather than claim that my analysis rebuts his (for after all, I too am describing a series of inquiries into Life which are not constituted as biology), I will simply that my concern with Life as an object of controversy and/or crisis is not found in Foucault's discussion, nor is it affected by it. For an excellent, less tendentious discussion of the shifting meanings of ‘biology’ and its predecessors, ‘physiology’ and ‘natural history’, and an analysis of the relation between ‘philosophy’ and these terms, see Gayon 1998. For the newer view that the eighteenth century was significantly concerned with ‘vital’ matters, see Reill 2005, which contains in-depth analyses of Buffon, Barthez and then Herder and the Humboldts, but is confusing taken as a whole since these figures do not seem to easily fit in one narrative.

10. The classic, and influential statement of this view is Loeb 1912; see also Smith 1976 for suggestive, but inconclusive discussion.

11. Buffon 1749, II, *Histoire générale des animaux*, Chapter 1, “Comparaison des animaux et des végétaux,” in Buffon 1954:238a–b; *ibid.*: Chapter 2, “De la reproduction en général,” in Buffon 1954:245b.

subject to laws that are “essentially different from” the laws of inert matter in motion; as sources, he refers both to Buffon and to the errors of iatromechanists with respect to the functioning of the “animal economy” (Venel 1753:410).

Lastly, in addition to this revision of the notion of Enlightenment, our question also raises a specific disciplinary issue in close relation to point (ii) above:

- (iv) Does this emerging ontological concern about Life reflect the constitution of a science? Is it a precondition for a science? The concern and its various verbal expressions clearly predate the coinage of the word ‘biology’ in German and French (and its establishment as a science) by roughly a century. (Salomon-Bayet 1981; Caron 1988)

As I will try to show in closing, the emergence of a ‘field of controversy’ concerning the status of Life is hardly synonymous with the constitution of the science called biology. That does not mean it is not productive of forms of knowledge, such as medicine or natural history or in a very different kind of categorization, ‘knowledge of the body’ (see Wolfe and Gal 2010). But by the time the name ‘biology’ (or its close competitor, ‘zoonomy’) comes to the fore as a “synthetic, unitary science of life” (Singer [1929] 1958:917) its concerns are quite different. Similarly, the question of the scientificity of medicine and anatomy in the late seventeenth century, or their disputed revolutionary status are again not to be confused with the existence of controversies over Life.

In what follows I survey (§ 2) some of the possible candidates for ‘controversies over Life’ in early modern natural philosophy, and argue that we should not think of them in these terms. I then turn (§ 3) to the context for where I do believe Life as a problem emerges – in the interaction between chemical and metaphysical debates concerning organisms, substance and fermentation, notably between Leibniz and Stahl. In a conceptual development which may seem rather counter-intuitive to contemporary readers, who tend to believe that “materialists explain everything in terms of matter and motion” whereas so-called animists like Stahl or vitalists like Bordeu explain everything “in terms of the soul or vital force” (Wellman 2003), the specific preoccupation with the nature of organic Life that characterizes Stahl is taken up – in materialist terms – by thinkers such as Diderot, as I discuss next (§ 4). Lastly, I ask, here and in the conclusion, how this materialist focus on Life did and did not become or at least lead to the constitution of biology as a science. My analysis spans the periods we call the Scientific Revolution and the Enlightenment, but it does not seek to either challenge or rehabilitate these terms as historically explanatory categories, although a reader with particular interest in such questions might notice that the emergence of Life as a locus of ontological crisis (or controversy) roughly matches the shift from the former to the latter.

2. Was life a controversial topic in early modern natural philosophy?

If we can speak of early modern life science, from physiology to theories of generation, from the chemical investigation of blood, aether and spirits to treatises on fermentation and fevers, then we can inquire into its relation to the constitution and stabilization of other parts of natural philosophy, such as mechanics and atomistic physics. Figures such as Harvey, Descartes and Borelli, or Boyle, Pitcairne and Malpighi, or Charleton and Boerhaave then loom large on the map and if our goal were to revise accounts of the Scientific Revolution so that they took account of such figures, it would seem reasonably easy to achieve. However, if we hoped to find scientific discoveries which contribute to a unified notion of physiological function, we shall not; if we take, e.g., one century of analyses of digestion, from Francesco Redi and Giovanni Borelli in the 1650s–1660s to René de Réaumur and Théophile de Bordeu in the 1750s, none of the experiments on gizzards and their grinding power, discussed by many naturalists, produce any unified result, until Lazzaro Spallanzani conducts experiments on digestion in the 1780s (Salomon-Bayet 1978: 336f., 342–343, 355f., 348).

But, as I have suggested earlier, another problem arises, which is less easy to resolve. In the early 1700s Georg-Ernest Stahl, a court physician to Duke Johann Ernst of Saxon-Weimar and subsequently, as of 1694, a Professor of Medicine at the University of Halle, stated bluntly: in all these competing theories of the human body, notably the very successful mechanistic theories, “*Life* was never mentioned nor defined, and I could find no logical definition provided” (Stahl [1706] 1859, vol. 2: 224). To follow Stahl’s suggestion, we could say that Life is either discussed but immediately dissipated into the entities and processes which subserve it, or promoted to the extent that vital spirits, vital heat, and animation are so co-extensive to the field of investigation that Life as problem again dissipates into the analysis as a whole. There is discussion, but no controversy, in the sense that there is no polarization between Life and non-Life (with the notable exception of Stahl in his polemic with Leibniz, which I shall turn to in Section 3), nor the possibility of resolution between two positions, which implies *some* shared conceptual framework (Freudenthal 1998); resolution or conciliation requires at least the sort of quasi-paradigmatic framework into which particular cases such as circulation, generation, monsters can be fit, as will happen with Haller in the later eighteenth century. Let us consider some representative examples.

Boyle’s corpuscularianism, despite its experimental attention to living bodies, the ‘history of human blood’, respiration and so forth, holds that both living and nonliving things are arrangements of a single universal matter, which is made up of corpuscles. Boyle does not want to trace vitality back to a faculty or a power, but rather to a certain arrangement of particles; hence vital processes may

be considered as separations and re-combinations of material corpuscles. Blood does not possess an innate faculty which makes it 'alive' but, like sweat or snow, possesses its specific chemical properties "by virtue of the motion, size, figure and contrivance of [its]own parts," and new qualities are produced by "changing the texture or motion" of bodies' constituent corpuscles."¹² Now, in an interesting passage in his *Disquisition About the Final Causes of Natural Things* – a work in which Boyle describes the human body as a 'hydraulic-pneumatic' machine – he appears to grant that there is a kind of category difference between "Living Animals" and "Dead ones", in which the latter are more like stones, possessed simply of a static structure. But then he quickly returns to his 'micro-mechanical' view (even if, *qua* corpuscularian his mechanism is an enhanced mechanism possessed of chemical properties and explains that the difference lies not only in the innumerable "Liquors, Spirits, Digestions, Secretions, Coagulations" but also in the "Motions" of the body and its limbs, which are present in living bodies but not in dead ones; in other words, a purely structural difference (Boyle 1688: 74–75).

If we turn to mechanism, including its medical variant, iatromechanism (leaving aside here the irreducible variety of forms of mechanism, the incommensurability of their types of explanation, including the possibility that iatromechanism may have been "simply irrelevant to biology", and the distinction between a mechanistic *ontology* and a mechanistic *method*¹³), we might expect to find a straightforward elimination of vital properties in favour of size, shape and motion, including the classic rejection of final causes which is a mark of the Scientific Revolution, if we think of Galileo's description of the 'fool' in his *Dialogues*, Simplicio, as a *cause-finalier*, who thinks that horses are on earth for the sake of man, grass is for horses, clouds and rain are there for the grass (Galileo [1623] 1953: 71), or Bacon's exclusion from "Physick" of explanations such as "the firmness of the skins and hides of living creatures is to defend them from the extremities of heat and cold : or, ... the leaves of trees are for protecting the fruit" (Bacon [1605] 2000: 86–87), and of course his 'barren virgins'. Equally well-known is Spinoza's contempt for the human ignorance which projects concepts of purpose onto the natural world, expressed notably in the appendix to *Ethics* I, where he asserts that "final causes are nothing but human fictions", and the preface to *Ethics* IV, where

12. Boyle, *Origin of Forms and Qualities* (1666), in Boyle 1772, vol. 3: 13; see also *Some Considerations Touching the Usefulness of Experimental Natural Philosophy, Second Tome* (1671), in *ibid.*, vol. 3: 427; Hall 1969, vol. 1: 294.

13. For the former possibility see Westfall 1971: 104; for the latter distinction see Des Chene 2005: 249–250.

he writes that what we call final cause “is nothing but a human appetite” – the causes of which we tend to be unaware of (Spinoza [1675] 2002: 239–240, 321).¹⁴

It is known that the exclusion of final causes and thus purposive vital faculties, but also humors and elements (Anstey 2011), in favour of a mechanistic scientific program produced notable drawbacks, such as the difficulty in accounting for epigenetic processes. Thus Descartes, who actively promoted the use of mechanical models as heuristics in studying the body, famously admitted his inability to account for the processes of generation in terms that were compatible with the mechanistic program he had set out for himself: “The formation of all the parts of the human body ... is something so difficult that I dare not attempt (to explain it) yet” (Descartes to Elisabeth, May 1646, AT, IV: 407); as Dennis Des Chene comments: “Among the phenomena of life, generation offers, along with the apparently reasoned behavior of higher animals, the greatest challenge to a science based on Cartesian principles” (Des Chene 2003: 413). Indeed, Descartes’ ‘failure’ to explain generation was notorious in the eighteenth century, e.g. Réaumur in the *Art d’éclorre des poulets* said that it was worse than if Descartes had failed to explain the universe (Gasking 1967: 68). This much is well known; the point I wish to emphasize is that everything ‘vital’ is necessarily excluded from mechanistic models, ironically given Descartes’ repeated insistence on health as an ultimate value and his ultimate, post-Cartesian doubt insistence on the survival value of our sensory organs. This exclusion is manifest in most of the celebrated pieces of iatromechanist propaganda, from Baglivi and Boerhaave:

Since Physicians began to examine the Structure and Actions of a living Body, not by Physico-Mechanical and Chymical Experiments, but by Geometrico-Mechanical Principles, they have not only discovered an infinite number of things that were unknown to former Ages; but have made it out, that a Human Body, as to its natural Actions is truly nothing else but a complex of Chymico-Mechanical Motions, depending on such Principles as are purely Mathematical. For whoever takes an attentive view of its Fabrick, he’ll really meet with Shears in the Jaw-bones and Teeth, ... Hydraulick Tubes in the Veins, Arteries and other Vessels, a Piston in the Heart, a Sieve or Straining-Holes in the Viscera, a Pair of Bellows in the Lungs, ... Pulleys in the Corners of the Eyes. And tho’ the Chymists explain the Phaenomena of natural Things, by the Terms of Fusion, Sublimation, Precipitation &c. And so make a separate sort of Philosophy; yet all these

14. That Boyle, in his work on final causes which I mention, as well as Leibniz explicitly do *not* reject final causes does not make their denial by Galileo, Bacon, Descartes, Spinoza any less canonical for the Scientific Revolution (or indeed its ideological inheritance from Fontenelle and Voltaire onwards).

ought to be imputed to the Force of a Wedge, Balance, Leaver, Spring, and such like Mechanical Principles ... the natural Effects of an animated Body can't be accounted for with greater Facility and Clearness any other way.

(Baglivi [1696] 1704: 135–136)

and

The solid parts of the human body are either membranous Pipes, or Vessels including the Fluids, or else Instruments made up of these, and more solid Fibres, so formed and connected, that each of them is capable of performing a particular Action by the Structure, whenever they shall be put into Motion; we find some of them resemble Pillars, Props, ..., some like Axes, Wedges, Leavers and Pulleys, others like Cords, Presses or Bellows; and others again like Sieves, Straines, Pipes ...; and the Faculty of performing various Motions by these Instruments, is called their Functions, which are all performed by mechanical Laws, and by them only are intelligible.

(Boerhaave [1708] 1751: 81)

One can see why Stahl protested that Life had vanished from the bio-medical purview. Indeed, in a kind of unconscious echo of Stahl's concerns, Boerhaave declared in a much-cited lecture on the "use of mechanical methods in medicine" that "the human body is in its nature the same as the whole of the Universe" (Boerhaave [1703] 1983: 96), which I take less as a Renaissance-type statement of correspondences and more in the sense of a broadly mechanistic commitment to an ontology in which material particles and their interaction exhaustively account for the physical universe including ourselves.

In fact, these apparently pure statements of iatromechanism mask a more complex (and concrete) reality on the ground, where functional dimensions are never wholly absent from physiological explanations. Even Descartes, in a 1646 letter to Elisabeth will speak of the "office" of the liver (Descartes 1964–1976, IV: 407), and chemical explanations as used in medicine by figures such as Thomas Willis and Stahl blend, if not seamlessly, quantitative and qualitative definitions of fermentation, such that one no longer knows what is a strictly particulate explanation versus one on which invokes 'liquors', 'juices', 'heat', 'spirits' and so forth. Consider for instance this elegant statement on the body from Bernard de Fontenelle in 1707, presented ostensibly in the context of a discussion of the pituitary gland:

The human body considered in relation to an infinite number of voluntary movements it can perform, is a prodigious assemblage of Levers pulled by Ropes. If one considers it in relation to the motion of the liquors it contains, it is another [sort of] assemblage of an infinite number of Tubes and Hydraulic Machines. Finally, if one examines it in relation to the production of these liquors, it is an infinite assemblage of Chymical Instruments or Vessels, Filters, Distillation Vats,

Receptacles, Serpentine, etc. ... The greatest Chemistry apparatus of all in the human Body, the most wonderful Laboratory is in the Brain, from whence this Extract of the blood is drawn known as Spirits, the sole material motors of the entire Machine of the Body. (Fontenelle [1707] 1730: 16)¹⁵

Regardless, we cannot view these different variations on the mechanistic program as comprising a science of life – or if so, then it is one in which Life as in issue is completely absent. One notable exception to this narrative of the ‘absence of Life’ is Francis Glisson’s *Tractatus de natura substantiae energetica, seu de vita naturae ejusque tribus facultatibus perceptiva, appetitiva, motiva* (Glisson 1672), usually referred to as *De vita naturae*. After publishing various significant medical works, such as *De rachitide* (1650) and *De anatomia hepatis* in (1654), Glisson produced this treatise on the “life of nature,” describing life as immanent to matter: “life is the intimate and inseparable essence of matter” and “matter contains within itself the root of life.”¹⁶ Now, it would be easy to dismiss this as a kind of substance metaphysics, as indeed Albrecht von Haller did when he both credited Glisson with the discovery of the property of muscular irritability and excluded him from the history of science proper (Giglioni 2008); but clearly Glisson reflects on the nature of our organic structure (*organizatio*, § 11), its relation to our sense organs, and how animal spirits are not a sufficient explanation of the features of ‘animation’ and complex perception which our sense organs display.

However, it remains a challenge to integrate this aspect of Glisson into a Scientific Revolution narrative, since it is rather a species of matter theory; what is more, this very immanentism means that the nature of Life does not arise as a topic for controversy for Glisson. A converse attempt has been made recently by Guido Giglioni to present the existence of a ‘vitalistic’ strain no longer in a marginal but in a central figure, Francis Bacon, focusing on the theme of the ‘appetites of matter’ and the related fixation on the “prolongation of life” (Giglioni 2010; 2005). But on the issue of the demarcation of Life as an object (a) that requires a specific science or group of sciences and (b) which existing sciences do not adequately treat, it seems more relevant that when Bacon is outlining the contents of the *Sylva sylvarum* (published posthumously in 1627; in Bacon 1857, vol. II), he presents thirteen works as “physiological remains” (Bacon 1857: “Table of Contents”); out of these, seven concern minerals and six concern attractive force and

15. Translation mine (unless otherwise indicated all translations are mine).

16. Glisson 1672, § 8; I quote from a draft translation of *De Vita Naturae* by Guido Giglioni, which he was kind enough to share with me.

the transformations of inanimate bodies (even if Bacon discusses these in 'biological' terms). Thus Bacon does not attend to, or is not concerned with, a distinction between the living and the non-living.

Mechanism, corpuscularianism, Baconian natural philosophy (to which one could add Locke's Helmontian medical reflections but also his philosophical consideration of the Life that is the unity and identity of a plant, animal or a man¹⁷) do not address the question of Life; they do not see it as a problem, or *a fortiori* an ontological crisis. If it is historically insensitive to leave out the life sciences from all accounts of the Scientific Revolution, as so many do, it is also mistaken to completely gloss over the problem. Thus Harvey is sometimes simply described as a bona fide member of the intellectual construct called 'Scientific Revolution', as in Gasking (1967: 40): "Harvey ... tended to stress the importance of observation and experiment, an increased emphasis on which was a vital part of that change in outlook which is sometimes called the Scientific Revolution".¹⁸ Another way of sweeping the problem under the rug is to state, as Peter Dear has more recently, that there is "no reason in principle" to "ignore the sciences of life", since 'physics' in the early modern period is conceived as inquiry into nature in general (Dear 1998: 190). Something is missing from this picture. Some scholars, particularly in the humanities, would say that what is missing is *the body* – and an entire sub-discipline of cultural history has devoted itself over the past twenty-plus years to studying the historical constitution of the body, with particular attention given to its Renaissance and early modern formations (see Brown 1988; Turner 1984; Sawday 1995; Mandressi 2009; the section on 'Bodies' in Cooter and Pickstone 2000; Wolfe and Gal 2010). But what precisely differentiates a living body from a corpse – a leitmotiv in the concern with Life – is heavily determined by the natural-philosophical engagement involvement with chemistry. It is to this, via Leibniz, that I now turn.

17. See Locke [1701] 1975, II.xxvii.4–6 on the Life qua identity of plants, animals and humans in mereological terms. His last word on the question (*ibid.*, III.x.22) does not advance the issue very much: the term Life is obvious for everyone, but when one turns to the status of a plant in a seed, a chicken in an egg, or a dying man, its sense is harder to grasp.

18. Rather more à propos is that, whatever Harvey's epistemological approach towards circulation was (mechanistic? Aristotelean? empiricist? Paduan? hypothetico-deductive?), "he did not attempt to formulate any general laws of life on a purely mechanical basis" (Ackerknecht 1982: 114).

3. Machines of nature, ferments, and chemical metaphysics

Mechanistic approaches to Life should not be caricatured as they sometimes are, e.g. by Richard Westfall, who described medical mechanism as “the puppet regime set up by the mechanical philosophy’s invasion” (Westfall 1971: 104). Whether in its earliest phases or – most evidently – in its late and complexified form such as von Haller’s ‘micro-mechanical’ analysis of physiological structure, combining structural and functional explanations, these approaches are not blind to the nature of vital processes, but seek to heuristically model them, e.g. by the usage of automata, which Borelli cleverly described as having “a certain shadowy sameness (*umbratilem similitudinem*) to animals” (Borelli 1680, vol. II: § viii).

And yet something has changed by the time of Buffon and Diderot in the late 1740s. Life, ‘organized bodies’ (*corps organisés, organisierte Körper*) and gradually ‘organisms’ are everywhere. The *Encyclopédie* discusses matters pertaining to biological Life far more, proportionately, than its predecessor and inspiration of fifty years earlier, Chambers’ *Cyclopædia*. Conversely, the *Encyclopédie* has no article on Galileo (whereas Galileo features prominently, e.g. in Brucker’s *Historia critica philosophiae*, which is a major source of the *Encyclopédie*; Salomon-Bayet 1978: 384). In § 4 of his 1753 *Pensées sur l’interprétation de la nature*, Diderot gave an exhortatory dimension to this state of affairs, and declared that

We are on the verge of a great revolution in the sciences. Given the taste people seem to have for morals, belles-lettres, the history of nature and experimental physics, I dare say that before a hundred years, there will not be more than three great geometricians remaining in Europe. The science will stop short where the Bernoullis, the Eulers, the Maupertuis, the Clairaut, the Fontaines and the D’Alemberts will have left it... We will not go beyond. (Diderot 1994: 561)

Similarly, Buffon asserted in his methodological discourse “De la manière d’étudier l’Histoire Naturelle” that “mathematical truths are merely mental abstractions, which lack anything real” (Buffon 1749: 53). Diderot also gave an explicitly vital or biogistic inflexion to metaphysics, declaring in his commentary on Helvétius’ *De L’Homme* [1773–1775] that “It is very hard to think cogently about metaphysics or ethics without being an anatomist, a naturalist, a physiologist, and a physician” (Diderot 1994: 813; Wellman 1987: 89, n. 43). What were the roots of this ‘vital’ change? Conceptually, Leibnizianism plays a key role – not necessarily the substance metaphysics of Leibniz as he intended it, but the series of deliberate, materialistically and/or biogistically inclined misreadings that were produced notably in France during the early Enlightenment, when thinkers such as Maupertuis, Bordeu and Diderot explicitly make use of concepts such as the monad but turn them into descriptive tools for the theory of generation, deliberately

ignoring Leibniz's own distinction between the physical and the metaphysical (Canguilhem 1980; Wolfe 2010). In a sense it is ironic for this turn towards Life to involve Leibniz so strongly, since he notably described organisms as 'machines of nature', which are machines down to their smallest parts and rejected extra-causal, mysterious vital forces. What did he mean by this?

Leibniz, like Aristotle, drew heavily on his observations (and reports from microscopists such as Leeuwenhoek) concerning living beings in the formulation of his metaphysics of substance. It is not that monads possess uniquely vital properties, but that their definition is inspired by the self-maintaining, self-regulating, autonomous features of living beings. In addition, Leibniz seems to have coined the term 'organism' in a technical sense to mean a type of entity different from machines, and synonymous with 'organized body'. (I say 'technical sense' because the term is used even after Leibniz, e.g. in the *Encyclopédie*, where it does occur, contrary to the claims of earlier scholars, in a yet undefined sense of the word, as synonymous with 'mechanism'.¹⁹) But mostly, Leibniz speaks of living beings as "machines of nature." The term first appears in his *New System of Nature*, published in 1695 in the *Journal des savants*. Machines of nature are machines in their "most minute parts" ("moindres parties"), contrary to machines created by human artifice (Leibniz 1978, vol. 4: 482); they are machines to infinity also in the sense that bodies contain seeds which can never be destroyed (ibid.: 475). He also specifies that it is living bodies which are machines of nature (*Monadology*, § 64 and for a full discussion of this notion in Leibniz, Fichant 2003). This is where the terminology of 'organism' starts to appear: "The organism of a living being (*organismus viventium*) is nothing other than a divine mechanism which is more subtle than an ordinary mechanism in the infinity of its subtlety" (Leibniz [1903] 1981: 16; Leibniz 1978, vol. 1: 15). Due to the law of the conservation of force among other reasons, Leibniz refuses to allow for any type of extra-causal influence on bodies of a vital principle that would be separate from bodies as a whole. Hence he denies a concept of soul as the motive force or controller in the body, which is what Stahl put forth. Leibniz insists that everything that happens in Nature happens according to mechanical laws. Of course, Leibniz also holds that Life stems from a "deeper source" than the ontological level of mechanically specifiable Nature, which remains at the level of passivity (letter to Hoffmann, 27 September 1699, Hoffmann 1749: 49a–b).²⁰

19. In the *Encyclopédie* articles "Fibre" and "Nutrition" (VI: 670; XI: 288) the terms "mécanisme" and "organisme" are used interchangeably, e.g. "the mechanism or organism of nutrition."

20. Discussed in Duchesneau 1982: 82; Leibniz's reply to Stahl in Stahl [1720] 1864: 14. For further discussion of the Leibniz-Stahl exchange as precisely a *controversy* see Carvallo 2010.

This aspect of Leibniz – that there is something unique about living beings, and this uniqueness is metaphysically grounded – was strongly brought to the fore by a series of his disciples precisely concerned with ‘biophilosophy’ and, increasingly, with the difference between organic and inorganic entities: Louis Bourguet in the 1720s and, better-known, Charles Bonnet a generation later. Bourguet, in the course of an extensive analysis of crystals, developed an original notion of “organic mechanisms” (*mécanismes organiques*) which functioned in a different way than ordinary mechanisms, and directly influenced Buffon’s idea of “organic molecules”. He suggested that there was a difference between the growth of crystals by juxtaposition, or the “apposition of new parts”, and the organic process of *intussusception* by which new molecules are integrated into the organic body and form a part of it, a distinction repeated almost exactly in the second half of the eighteenth century by Linnaeus, Bonnet, Lamarck and others.²¹ The distinction between ‘apposition’ and ‘intussusception’ is between two forms of growth, the former characteristic of minerals (such as crystals), and the latter characteristic of plants and animals, which is the “intussusception of a new matter” (Maupertuis 1746: 44). Bonnet plays on the French word for organic growth (the verb *croître*) and says that crystals merely agglomerate (*accroître*) rather than actually grow (*croître*) (Bonnet 1768, vol. 1, ch. XII, §§ 170, 210: 143, 189, 191). Bourguet describes “organic mechanisms” in Leibnizian terms as a combination of various types of molecules – from aether, water, earth, air, etc. – which are subordinate to a “dominant Monad or Activity” (Bourguet 1729, 4th letter: 164–165). Bonnet explicitly declares that “nutrition, development and the formation of a new organized being are the products of an *unknown force* ... which has nothing in common with *mechanical forces*” (Bonnet 1764: 92; italics mine) and more humorously, that “I have always led my reader back to the Being of beings, and shown his handiwork in all the products which have falsely been traced back to purely mechanical causes, as if an animal had the same origin as a cheese”²² (Bonnet was Swiss, after all).

But let us return to the discussion with Stahl, since it is essentially here that Leibniz develops a concept of organism, because Stahl, in a combination

21. Bourguet 1729, 4th letter, 73, 165–166; see Cheung 2006, § 2; also Duchesneau 2003; Linnaeus, Introduction to the *Systema naturae* (and of course in his celebrated aphorism that ‘Stones grow, plants grow, and live, animals grow, live and feel’, in the *Philosophia botanica*); Lamarck in the *Système des Animaux sans vertèbres*, discussing the formation of the shell in mollusks (Lamarck 1801: 55).

22. Letter to Malesherbes of October 30, 1762, quoted in Savioz 1948: 214; Bonnet is admittedly protesting against the ban of his *Considérations*.

of medico-physiological and chemical reflection, insists repeatedly on *Life*. To put it differently, the recognition of Life as a problem (which goes hand in hand with the formulation of 'organism' as a concept) is an effect of Leibniz's debate with Stahl, since their disagreement specifically centres on Stahl's assertion that the organism obeys causal laws which are different from those operating in mechanical nature overall, an assertion Leibniz cannot accept *although* he too wishes to defend a concept of organism (Duchesneau 1995; Carvallo 2010). And, especially on Stahl's part, the conceptualization of what an organism is and how it differs from a mechanism (or, which is much the same, how a living body differs from a dead body) centrally involves chemistry (or 'chymistry'²³); hence Life becomes an object of controversy. I shall now discuss this chemical contribution to the emergence of Life as an ontologically problematic entity (notably with the concept of fermentation, and the consequent role of analyses of digestion) before turning, in Section 4, to the radical materialist appropriation of these inseparably chemical and 'biophilosophical' elements, and its relation to the constitution of 'biology'.

If we recall Bourguet's distinction between the formation of crystals by juxtaposition of their components, versus organic entities which are formed by the intussusception of their molecules, the issue with chemistry – particularly the notions of fermentation and 'seeds' (what Gassendi called, using a Lucretian term, *semina rerum*; the difference is that for Lucretius these 'seeds' were simply atoms, whereas for Gassendi they were composites or compounds of atoms²⁴) – revolves around a distinction between beings that are merely 'formed' and beings that are 'generated'. It is only in the late seventeenth century that this distinction becomes crucial – both because the list of candidates for the latter gradually gets defined more narrowly, so it can no longer include metals, crystals and minerals, and because the iatrochemical Paracelsian–Helmontian–Sylvian–Stahlian tradition of

23. The term 'chymistry' is increasingly preferred in current scholarly usage, as it emphasizes the absence of a non-arbitrary and historically justified analytic division between 'chemistry' and 'alchemy' in the early modern period. While there was certainly a range of theories and practices in the science of matter, 'chemistry' and 'alchemy' do not pick out a meaningful division within that range, and so the term 'chymistry' is used as a general term for all such theories and practices, following Principe and Newman 1998. (Thanks to Lydia Barnett whose work helped me see this more clearly.)

24. Bloch 1971:252, n.75. On the shifting meanings of vital *minima*, notably 'molecules', in seventeenth-century chemistry, matter theory and philosophy see, in addition to Bloch 1971, Clericuzio 2000:63–71, and for the impact of *semina rerum* on early modern matter theory overall, Hirai 2005.

‘chymistry’ strongly focuses on the processes that are unique to organic beings, such as fermentation.²⁵

A major source for this idea of fermentation is Thomas Willis (1622–1675), who taught natural philosophy at Oxford and medicine in London; he was best known perhaps for his discovery of the ‘circle of Willis’ and his great work on the anatomy of the brain, *De cerebri anatome* (1664) (richly illustrated by Christopher Wren). But the work that concerns us is his *De fermentatione* (1659), translated as *A Medical-Philosophical Discourse of Fermentation; or, Of the Intestine Motion of Particles in Every Body*. *De fermentatione* was meant to be the introduction to his theory of fevers, which in fact he explained as the outcome of a vitiated fermentation of blood (Willis also says that he added a treatise on fever to the one on fermentation in order to apply his fermentation theory to fever). “Every disease acts its Tragedies by the strength of some Ferment” (Willis; in Debus 2001:69). Is fermentation chemical or mechanical? The iatrochemical answer should be straightforwardly the former, since it describes all material bodies as being composed of the principles of Spirit, Sulphur, Salt, Water and Earth and the mixture and proportion of these.²⁶ But Willis complicates matters by sometimes speaking of fermentation in more purely iatrochemical terms, sometimes in more mechanical terms, as a motion of the parts. Ferments helped kindle the particles of spirit and sulphur in the blood into a flame, a combustion that was also called effervescence of the blood, which is how Willis explains body heat and fever. The fermentation in the heart heats the blood like “Water Boyling over a Fire”,²⁷ and this heat is distributed to the whole body through blood circulation, constituting the common cause of ordinary body warmth as well as febrile heat. Our body heat is the effect of a chemical cause – specifically, of fermentation.²⁸ And, most

25. I thank Justin E. H. Smith for this suggestion. Joly 2004 observes that Renaissance and early modern chymical treatises frequently describe minerals and metals in terms which we would only use for living beings (“seeds and germination, growth and rot, death and resurrection”); he suggests that this is less because the chemists were intellectually chaotic and more because a doctrine of living being was simply absent.

26. According to Clericuzio (ms., 2009), spirits, sulphur and salts are the active principles (with spirits being the most active), while earth and water are passive. Spirits affect various properties of the body, from heat to conservation to preventing putrefaction; e.g., the digestive system is described as a process of fermentation in the stomach. For a discussion of chemical vs. mechanical explanations of fermentation, see Mendelsohn 1964:380; Chang 2002: 56, 59f.

27. Willis, *Of Feavers*, in Willis [1659] 1681, Chapter I, § 1: 59 (pagination continuous with *Of Fermentation*); Chang 2004:785.

28. Bates 1981 suggests that “for Fernel [and all traditional Galenists], the essence of fever was preternatural heat whereas for Willis it was an inordinate motion of the blood” (49).

relevantly for us, “The first beginnings of life proceed from the spirit fermenting in the heart” (Willis [1659] 1681, Chapter V: 13).

Stahl, too, viewed the body as composed of organic matter in a process of fermentation, which in fact meant it was vulnerable to putrefaction – indeed, always in a process of putrefaction in some sense. Some parts of the body are more vulnerable than others, notably the blood; hence Stahl describes circulation as a process which preserves the *mixtio* of the blood and thereby maintains the stability of the whole. This is a good example of how his system renders the chemical and the metaphysical almost indissociable, in his description of the living body as a kind of dynamic equilibrium which constantly has to be maintained. On the one hand this equilibrium is chemically specified, both at the level of the concept of fermentation and with the description of the body as a chemical *mixtio*, not a mere aggregate: since aggregates are merely mechanical combinations of portions of matter in motion, whereas mixts imply a notion of qualitative diversity above and beyond the spatial proximity between particles. (This distinction, which is crucial for Stahl, will be wholly appropriated by Diderot who uses it to define the relation between matter in general, living, sensing individual molecules and the ‘sensitivity of the whole’). Leibniz reiterated this in his own terms: “a mass of matter is not properly what I call a corporeal substance, but rather an aggregate of an infinity of such substances, like a pack of sheep or a pile of worms.”²⁹ On the other hand, Stahl famously describes the body and its organs as literally mere instruments of the soul, a position sometimes revised so that “organs are not, as the name might suggest, mere instruments”, but nevertheless, “it is the soul that makes the lungs breathe, the heart beat, the blood circulate, the stomach digest, the liver secrete” (Stahl 1859: 347). Put these two together and you have the notion of a “highly fermentable organic body [which] has to rely on a vigilant *anima* to discharge the corrupt and harmful materials from the vital economy in a timely manner” (Chang 2002: 63).³⁰

These concepts of fermentation are closely linked to concepts of Life, and indeed were actively taken up in the second half of the eighteenth century by vitalists, notably when dealing with the phenomenon of digestion. Whereas mechanists or ‘solidists’ (who held that illness is due to a pathological change in the solid parts of the body, as opposed notably to humoralists), but also Newtonian-inspired physicians including Boerhaave and Pitcairne reduced digestion to a quantitative process of ‘trituration’, of spatial relations between masses, or more

29. Leibniz, “Éclaircissement sur les Natures Plastiques et les Principes de Vie et de Mouvement” (1704), in Leibniz 1978, vol. 6: 550.

30. It’s not always so clear-cut, though, because chemistry for Stahl is both something foreign to the *theoria medica vera* and nevertheless that which explains life (Roger 1979: 45).

specifically the expansion and contraction of muscles, iatrochemical physicians and post-Stahlian chemists like Venel – in his articles for the *Encyclopédie* including “Chymie”, “Chaleur”, “Digestion” and “Mixte” – emphasized the chemical transformations of the substances involved in the digestive process. Thus François Boissier de Sauvages, in his *Nosologie méthodique* of 1763, explains that the faculties of the body are equivalent to the properties of matter in general (e.g. gravity, elasticity and attraction), but that *within the organism* these produce processes of fermentation and putrefaction which seem to be restricted to living beings (Sauvages [1763] 1771, I, §§ 150–154, 261, 266).

Of course, the mechanical explanations of digestion are augmented with processes such as heat, vibration, the action of the spirits, and continuous compression, recalling our earlier point that it is not always appropriate to fully distinguish the ‘mechanical’ and the ‘purposive’ or the ‘functional’, either in seventeenth- or eighteenth-century physiology and natural philosophy. Sauvages believed that the fully self-contained nature of his calculations on the body’s energy proved the existence of an independent soul which was the source of this motion, and – surprisingly, we would think, for someone who stresses the causal role of the soul in vital functions (i.e., what came to be called an ‘animist’) – praised the discoveries of Baglivi, Pitcairne, Newton and Boerhaave precisely for their calculations as applied to the body (Sauvages 1731:2). As Roger French comments, “it is something of an ‘ism’ paradox that the eighteenth-century ‘mechanists’ generally described the body in non-quantitative terms whereas the ‘animists’ used mathematics to demonstrate the need of a soul to power the machine of the body” (French 1990: 103). A missing term in this opposition between mechanism and animism is vitalism.

Organisms, ferments and digestive systems all have some more or less obvious, more or less intuitive relation to an idea we might call Life, and indeed gradually, from the iatrochemists to Stahl, and onwards to his disciples in the mid-eighteenth century and their *Auseinandersetzungen* with the group of physicians who come to be called vitalists by the dawn of the next century,³¹ these kinds of phenomena, together with more broad research programmes such as physiology (as opposed to anatomy), are being presented as specifically vital. Venel, in the article “Chimie” in the *Encyclopédie*, speaks of “changes” which bodies undergo, such that they “move from the non-organic state to the organic state”, and suggests that the “phenomena of organisation [i.e. organism, organic phenomena] should be treated by a science separate from all other parts of Physic”, namely, chemistry (Venel 1753:410). Where is the crisis, then? What happened to the ontological

31. The word ‘vitalist’ appears at much the same time as does the word ‘biology’, a fact that has not so far been discussed much, if it all. On the history of the former, see Wolfe and Terada 2008; on the latter, see Caron 1988 and Barsanti 2000.

controversy? Remember that Stahl had spoken in fairly strong terms, if not of scandal then at least of shock: “What shocked me above all was that in this physical theory of the human body, *Life* was never mentioned nor defined, and I could find no logical definition provided” (Stahl [1706] 1859, vol. 2: 224). And throughout the collection of essays entitled *Theoria medica vera* (Stahl [1708] 1860), he asks about what we call Life and what purpose does it serve within and outside the body? Stahl is challenging a dominant, and vast set of views partly encapsulated under the notion of *mechanism*, or specifically *iatromechanism*. He does not respond with a coherent model, program, or unifying concept, such as ‘organicism’ which other physiologists and teaching physicians could both apply and improve on, as Haller precisely did with *his* physiological model (usually referred to as the Göttingen school; see Steinke 2005). Had Stahl done so we could possibly study various tensions between mechanistic medicine, physiology and chemistry and organicist medicine, physiology and chemistry as *bona fide* controversies.

From an external standpoint, as historians or onlookers in general, the problem is not so much to find a definition – at the present time we still have not agreed on a definition of Life, or what constitutes its exact origin (see Deamer and Fleischaker 1994) – as to understand why it becomes a problem and what the effects of this problem are. One response, a fairly rhetorical one, is that of the Montpellier vitalist physician Théophile de Bordeu: “Spare us, once and for all, all these tiny fibres, pressures, globules, thick substances, sharp angles, lymph, hammers and all the rest of the equipment from mechanical workshops with which [earlier doctors] filled the living body – they were the playthings of our fathers” (Bordeu [1764] 1818, vol. 2: 670). Another possible answer to the ‘why?’ question is suggested less rhetorically by Peter Hans Reill: “if mechanism could, e.g., explain the pumping action of the heart, it was incapable of saying why the heart continually kept pumping without running down” (Reill 2005: 135). Obviously, for Stahl a major part of the answer lay in the soul, and specifically its purposive, goal-directed action, a view which earned him the ridicule of many prominent scientists, such as Haller, who suggested that Stahl’s (who rejected interventionist medicine in the face of disease) were to mechanist physicians like a half-naked ancient German warrior was, compared with an armed Roman centurion, in uniform.³² One can also try and reconstruct Stahl’s often unnecessarily obscure argumentation in a charitable way, and point out that he never denies the basic laws of physics and chemistry, nor the fact that living bodies, too, obey the laws of motion. As we saw with fermentation, the idea is rather to articulate a

32. Haller 1751: 956 (a review of Volters’ *Gedanken von Psychologischen sachen*), as quoted in Reill 2005: 123–124.

kind of ‘emergentist’ view in the weak sense that certain arrangements of particles exhibit complex, goal-directed behavior.³³

However, Stahl is quite adamant that the above be attributed to the soul, which then controls the various mechanically specifiable parts of the body as so many *instruments*. Stahl is a teleologist, who definitely believes, not unlike his twentieth-century compatriot Wilhelm Reich (1968:45), that “The question, ‘*What is life?*’ lay behind everything I learned. Life seemed to be characterized by a peculiar reasonableness and purposefulness of “instinctive involuntary action”. But all of the new chemical concepts he both appropriates and innovates with, allow for both an anti-reductionist interpretation (his own), and a reductionist interpretation – which is not specifically mechanistic, as we shall see – with materialists such as Buffon and Diderot. And in this reductionist approach, the vital dimension is not discarded.

4. Constitutive materialist ontology of life or gradual constitution of biology?

Neither biology nor chemistry exist as stable theoretical entities in the early modern or Enlightenment periods, even if chemistry had existed for a long time, but on unstable methodological and conceptual bases. Yet the constitution of an autonomous ontological region corresponding to ‘the science of living beings’, i.e. biology, is significantly affected by chemistry, as we have seen. One way to describe this is to say that chemistry is, at least at this time, the science which “allows for an understanding of matter as something that – at least provisionally – cannot be reduced to calculation” (Starobinski 1999:86). Recall Buffon’s and Diderot’s anti-mathematical proclamations of a new science of Life (“natural history”, but also the study of the “animal economy” in medicine), or the prominence of vital matters in the *Encyclopédie*.

Iatrochemical, Stahlian concepts that merge the chemical and the metaphysical are turned into reductive materialist concepts by Diderot (reductive notably in the sense that they are meant to replace explanations that appeal, e.g. to the soul). Yet these concepts are not themselves meant to be mechanically or ultimately, mathematically specifiable. This takes several interrelated forms: Diderot’s enriched atomism of vital *minima*, in which the ‘atoms’ or ‘molecules’ of living matter are themselves *alive*; his transformation of Hallerian irritability via Bordeu’s

33. T. S. Hall describes certain theories of Life in the eighteenth century (notably that of Maupertuis) as ‘emergentist’ (Hall 1968, vol. 2:26–28); for additional discussion of how eighteenth-century models of organic life can be understood as ‘organizational’ and thus beyond the split between reduction and emergence, see Wolfe and Terada 2008: 558–574.

concept of sensitivity (*sensibilité*) into a concept of organic sensitivity which is itself a property of living matter. The difference between irritability and sensitivity in Haller is that the former is fully mechanically specifiable and is strictly a property of muscle fibres, while the latter has a functional component as it is directed towards the organism's survival, and it presupposes the existence of the 'soul'.

In Diderot, this difference is collapsed into one property of living matter, with some waverings as to whether this property occurs in the elements or only in organized wholes, but he seems to opt ultimately for the latter. Sensitivity and therefore Life require, according to Diderot, the presence of organic "continuity" rather than mere spatial "contiguity" (Diderot's terms, which map onto Stahl's distinction between aggregate and mixt; Diderot 1994: 625–628; Boury 2006; Wolfe 2006). The difference between the life of an organic being and the life of a wooden automaton, or a watch, is not that the former possesses a soul, or is free, whereas the latter is not. The difference is, one might say, a structural one, between two different types of arrangements of parts. This is what Leibniz, a favourite author of Diderot's, meant when he declared that "a feeling or sensing being is not something mechanical like a watch or a windmill" (Preface to the *New Essays*, in Leibniz 1978, vol. 5: 59) or, in Diderot's version, which reflects his annoyance with the prevalent clock metaphor: "What a difference there is, between a sensing, living watch and a golden, iron, silver or copper watch!" (*Elements of Physiology*, in Diderot 1994: 1283).

It is for this reason that the concept of 'mechanistic materialism' is so problematic, and perhaps downright false (Kaitaro 1987): because most materialists, unlike Descartes, do not claim that physical nature is essentially specifiable in mechanistic terms. Diderot's challenge is to be able to do justice to the difference between organic and inorganic beings, without having reference to a concept of 'soul', anima, as the basis of animation, given that the distinction between 'animate' and 'inanimate' initially means 'possessed-of-soul' versus 'not-possessed-of soul' (Cunningham 2003: 58). This will be the concept of active, sensing matter. Hence his materialism is significantly focused on the concept of Life. It is in this sense that his "revolutionary" fervour (the *Thoughts on the Interpretation of Nature's* "We are at the dawn of a revolution", Diderot 1994: 561) is not just a way of participating in the emergence of biology as a science, since it is also a philosophical project. Consider the article "Spinosiste" of the *Encyclopédie*, by Diderot:

Spinosist: follower of the philosophy of Spinoza. One must not confuse the ancient Spinosists with the modern Spinosists. The general principle of the latter is that matter is sensitive; they demonstrate this by the development of the egg, an inert body which by the sole means [*instrument*] of graduated heat moves to the state of a sensing, living being, and by the growth of any animal which in its inception [*principe*] is merely a point, and through the nutritive assimilation of

plants and – in one word – of all substances that serve the purpose of nutrition, becomes a great sensing and living body in a greater [expanse of] space. From this they conclude that only matter exists, and that it is sufficient to explain everything. For the rest, they follow ancient Spinosism in all of its consequences.
(*Encyclopédie*, vol. XV: 474; Diderot 1994: 484)

No one has ever produced a satisfactory explanation as to why Diderot chooses to place an affirmation of his biologically motivated metaphysics within an entry on a philosopher (or a derivative of the philosopher) who did not himself think there was anything metaphysically unique about living beings. The first scholar to call attention to it, Paul Vernière, invented “neo-Spinozism”, as a category to describe precisely this biologically reconceptualized Spinozism. Vernière meant by this a form of holist materialism founded on the life sciences rather than on a priori metaphysical speculation (Vernière [1954] 1982: 529); what “neo-Spinozism”, which he attributes not just to Diderot but to Maupertuis as well, does is “refashion a monism more in accordance with the findings of science” (ibid.: 533). In the present context I will content myself with the observation that the difference between “ancient Spinosists” and “modern Spinosists” effectively maps onto the historical narrative I have been suggesting: whereas ancient Spinosists are essentially substance metaphysicians, their modern descendents are essentially focused on Life, specifically, the radical implications of the biological theory of epigenesis.

Harvey, who is supposed to have coined the term, defines epigenesis as “the superaddition of parts out of the power or potentiality of the pre-existent matter” (Harvey 1653: 223). More specifically, epigenesis is the theory of generation (or development as we would now call it) in which the characteristics and structure of the mature organism may be pre-determined in the embryo, but are not “pre-imprinted” in it. Rather, they are acquired during the course of a gradual development, in which the embryo undergoes transformations under the influence of the environment. In this sense, it is opposed to the preformationist theory, according to which all the characteristics of the developed organism correspond directly to characteristics “imprinted” in the embryo. Thus ‘epigenesis versus preformationism’ seems like the basis for a controversy in the theory of generation. We might think that we have finally encountered a proper controversy over Life; but in fact it does not become one until Diderot takes hold of it. Or rather, to introduce a distinction, if ‘preformationism versus epigenesis’ is a controversy in the life sciences, beginning fitfully in the early 1700s in the *Académie des sciences* and reaching full velocity with Haller, Wolff, and Blumenbach in the late years of the century, what happens with Diderot is that it becomes, precisely in the wake or rather the vein of Stahl, an ontological controversy. This is most explicit if we add to the entry “Spinosiste” a passage from *D’Alembert’s Dream*, the first sentence of which François Jacob in fact used as an epigraph for his *La logique du vivant* (Jacob 1970):

Do you see this egg? With this you can overthrow all the schools of theology, all the churches of the world. What is this egg? An unsensing mass, prior to the introduction of the seed [*germe*]; and after the seed has been introduced, what is it then? Still an unsensing mass, for the seed itself is merely an inert, crude fluid. How will this mass develop into a different [level of] organisation, to sensitivity and life? By means of heat. And what will produce the heat? Motion.

(Diderot 1994:618)

Aside from its stated radical dimension (to overthrow all schools of theology), there is also clearly something ‘vital’ about the commitment to epigenesis, or even vitalistic, since “All believers in epigenesis are Vitalists”, as Hans Driesch observed (Driesch 1914: 39; see Oyama 2010).

The transformative, ‘epigenetic’ dimension of living beings, which fascinates Diderot (“Voyez-vous cet œuf ?”) or La Mettrie (who uses Lucretian motifs to describe the living Earth as like a womb (*utérus*) which has now grown barren, so that new species do not arise³⁴), which also fuels the fascination with monsters, is very far removed from the set of possible criteria for the science of biology by the early nineteenth century, which include a reductive constraint on explanations of living beings in terms of their physico-chemical nature, a unification criterion which states that all living entities (including plants and animals) possess properties such as development, reproduction, nutrition, respiration, beginning in a basic substance (protoplasm) and ultimately arriving, by the middle of the nineteenth century, at the study of development, focusing on structure and function (morphology and anatomy versus physiology); at this point biology also requires cell theory in order to explain cellular division and conjugation, and has to incorporate evolutionary and ecological components (Singer [1929] 1958; Caron 1988; Barsanti 2000). It is no surprise that Cuvier by 1810 can declare that “the anatomical portion of the general problem of life has been resolved for a long time, at least as concerns the animals which interest us the most” (Cuvier 1810, “Histoire naturelle”, II:207).

To reiterate the point otherwise, the various instances of an emerging ‘life science’ in the eighteenth century, from the renewal of theories of generation to Haller’s work on irritability, to pieces of ‘folk biology’ such as Trembley’s polyp or Bonnet’s aphids (Roe 1981; Lenhoff and Lenhoff 1989) are not themselves identical with an ontological concern with the status of Life. To conflate these two would be to create a monolithic concept of vitalism which would somehow lead inexorably to the constitution of biology as a science. As much as Buffon, Haller, Barthez, Blumenbach, Bordeu, Venel, Diderot and especially Ménéuret de

34. La Mettrie [1750] 1987, Sections 8–11; he thinks the moderns can improve experimentally on this view, but does not reject it wholeheartedly.

Chambaud insist on the separation between life sciences and physical sciences – a separation which formally culminates in Bichat, Cabanis, and Bernard – it is not clear that all these figures, and certainly not Bichat or the ‘founders’ (or at least coiners) of biology Lamarck, Treviranus and Carl Christian Erhard Schmid (the author of *Physiologie philosophisch bearbeitet*, 1798–1801)³⁵ care about ontology. After all, no less a figure than the head of the Montpellier vitalists, Paul-Joseph Barthez, declared “I am as indifferent as could be regarding *Ontology* considered as the science of entities” (Barthez 1806, vol. 1: 96, n. 17).

It is true that some of these figures viewed these episodes of the coming-to-be of biology as not conforming to the laws of mechanics (or even violating them), and thus placing “in serious difficulty the traditional paradigm, based on the sovereignty of physics” (Barsanti 2000: 124). Thus the geologist Jean-Claude Delamétherie declared that the living being was “a machine that confounds all our ideas of mechanics”.³⁶ But the more a science of biology emerges the less it is concerned with the ontological crisis about Life. My interest here is more in the problematic status of Life in between historical categories such as the Scientific Revolution and the Enlightenment, and less to produce some ahistorical typology of controversies; but one is tempted to venture a distinction between an *ontologically* controversial moment (Stahl, and its materialist version in Diderot) and a *functionally* controversial moment that emerges with the positive science of life.

5. Conclusion

Why was there no controversy over Life in the Scientific Revolution? This episode, or absence thereof, is very difficult to make sense of in terms of familiar concepts such as ‘paradigms’ or ‘normal science’, for at least two reasons. First, since the sciences involved in the constitution of Life as a problem are not unified, discursively, experimentally or by their objects, whether we speak of alchemy, natural history, iatrochemical medicine or ‘metaphysical’ reflections on vital *minima* and the relation of body and soul; as Claire Salomon-Bayet nicely suggests, it would then have to be a “permanent revolution” (Salomon-Bayet 1978: 15; Salomon-Bayet 1981: 36, 39) lasting one hundred and fifty years (in her case the object is the study of life

35. Schmid 1798–1801, I: 140, cit. in Risse 1972: 153–154. See Schiller 1980: 85–87; Caron 1988: 231–232 for further indications on the early uses of ‘biology’, ‘zoonomy’ and the older ‘physiology’ to designate the same science.

36. Delamétherie 1787, II: 292, cited by Barsanti 2000: 124.

in the *Académie des sciences*; we could just as well say ‘from Harvey to Pasteur’³⁷). Second, by the time it (biology, medicine, etc.) is a science and is perhaps on the way to producing normal vs. abnormal patterns of discourse,³⁸ it has already lost its ontological dimension and thus its sense of being a crisis or a “scandal” – even if figures such as Hans Driesch at the end of the nineteenth century can effectively re-play Stahl versus mechanism, invoking Aristotelian entelechies just like Stahl did. This lack of fit between the various strands of the Life narrative in early modern science, and Kuhnian concepts such as paradigms, is sometimes used to demonstrate that Kuhn’s concepts might not work here (Salomon-Bayet 1981), even if some attempts have been made, unsuccessfully, to interpret notably Haller’s enhanced mechanistic model for physiology as precisely a “paradigm shift” (Toellner 1977). What it tells us here is not so much a matter for Kuhnian philology as a problem for understanding the development of biology as a discipline and how it relates to the more ontologically oriented discussions of the previous century, from the core years of the Scientific Revolution to the *Spätaufklärung*.

If, then, Life and the investigation into Life is not manageable as a Scientific Revolution narrative, we can of course revise the latter to include more discussion of animal spirits, of Newton’s queries on sensation and their influence on biomedicine in the next generation, and of course of the shift from a notion of ‘soul’ to various embodied and cognitive concepts. We can also insist on the presence of quantitative experimentation, notably in the Italian anatomists. But we will not be able to reconstruct a controversy over Life within the frame of the development of biology. In the seventeenth century Life is either everywhere, as in Gasendi or Glisson, but it is immediately dissipated into the entities and processes which mechanistically subserve it, or promoted to the extent that vital spirits, vital heat, ferments, seeds and other forms of animation are so co-extensive to the field of investigation that Life again dissipates into the analysis as a whole. There is discussion, but no controversy, in the sense that there is no polarization between Life and non-Life. In the eighteenth century, with Stahl and Diderot, Life becomes a ‘crisis’ concept – with anti-reductionist and reductionist trajectories, respectively – until by the early nineteenth century it resolves into being a structural concept with no ontological component. This is patently the case in Claude

37. Cuvier provides his own capsule history of the birth of life science *qua* science (i.e. as a system allowing for causal explanations): we have known the causal processes at work in digestion for centuries; the absorption of substances, since Pecquet, Rudbeck and Ruysch; the process of circulation, since Harvey. “The work of the English and Italian anatomists on the lymphatic system has ... completed everything that remained to be said in this regard” (Cuvier 1810: 208).

38. Say, Pasteur versus Pouchet on spontaneous generation or Cuvier versus Geoffroy Saint-Hilaire on the *plan d’organisation* (mentioned in Dascal 1998).

Bernard – a careful reader of Diderot, who left behind an unpublished manuscript on the latter's medical and physiological writings (Barral 1900) – for whom vitality is an effect of a particular type of physical organization, and nothing more: “l'élément ultime du phénomène est physique; l'arrangement est vital.”³⁹

We could conclude, following a hint of François Duchesneau's, that the concept of Life is an artificial construct, an *être de raison* created when rationality runs up against the speculative limits of a physiological theory that experience cannot wholly circumscribe (Duchesneau 1982:487). But what about the “revolutionary” force of epigenesis? The sense Diderot had that he and others were “on the verge of a great revolution in the sciences”, but not a revolution that was subsumed under an autonomous science of biology? As I have tried to describe, this “great revolution” which did not happen at least as envisaged by Diderot, combines the ontological *crisis* component of Stahl's animism with the new materialist focus on epigenesis and other key features of biological entities (“modern Spinosism”, as Diderot calls it). It is neither a feature of the Scientific Revolution nor of the Enlightenment per se; it is more of a *Sattelzeit*, Reinhart Koselleck's term for epochs which lie in between the recognized stages in our historical narratives, whether as transitions or as inassimilable moments (Zammito 2004). This *Sattelzeit* of Life as a scientific and philosophical problem, prior to the emergence of biology as a positive science in the nineteenth century, combines both a mechanistic interest in structures, components and ‘how things are put together’ with a recognition of the challenge created by new concepts of organism, body, animal economy etc.; and yet it does not hypostatize these concepts into transcendent entities beyond the reach of natural science. It is this radical, ontologically controversial aspect of Life that I have discussed here.⁴⁰

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39. Bernard 1879:524, cited and discussed in Métraux 2004:44–46.

40. This is part of a broader project to examine the relationship between materialism and embodiment in early modern science and the Enlightenment – moving away from stereotypical schemas such as ‘mechanistic materialism’ but also from certain theories of embodiment which oppose it to the concepts of natural science. See further Wolfe and Gal (eds) 2010, Huneman and Wolfe (eds) 2010, Wolfe and Terada 2008, Salter and Wolfe 2009.

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PART IV

Human sciences and theology

The pre-Adamite controversy and the problem of racial difference in seventeenth-century natural philosophy

Justin E. H. Smith

1. Introduction

One of the most intractable and divisive controversies of the modern period has revolved around the question as to the unity or diversity of the human species. How different are the various human sub-groups (sometimes called, unscientifically, ‘races’)? Do these differences extend to cognitive faculties and behavioural repertoires, or are they only skin-deep? What is it that explains the differences? Separate evolutionary pasts? Separate creations? Degeneration from an originally perfect and homogeneous state?

Over the course of the twentieth century, a solid scientific consensus emerged that effectively answered all of these questions: we now know that the genetic differences between any two members of a single ‘race’ are on average just as great as those between any two members of two different ‘races’. Problem solved. However, in spite of the often blatantly racist motivations of the modern European theorists of racial difference, the fact remains that prior to the twentieth century the question of the extent of racial difference was a question very much worth asking. The human species really is full of variety. This just happens to be a variety of individuals, but there is no *prima facie* reason why different human groups should not be unequal in various respects. If *homo sapiens* had not killed off the Neanderthals, after all, we would in fact be living in a world that includes a cognitively inferior human group.

At the beginning of the modern period, controversies having to do with the extent and the causes of human diversity were motivated by a genuine concern to make sense of the new ethnographic and physical-anthropological information that was flooding into Europe as a consequence of the rapid rise of exploration, trade, and colonization. This information challenged many deeply held beliefs about the unity of the human species. One of the most controversial

interpretations of this new information, the theory of polygenesis, effectively deprived Christian scripture of its supposedly universal authority by decoupling the origins of ‘pagan’ peoples from the account given in the book of Genesis of the origins of those humans whose generations extend back to Adam and Eve.

This theory is best known in its nineteenth century American expression. Here, its defenders held that the different races – but most saliently the ‘white’ and the ‘black’ ones – were created separately, and thus shared no common ancestry, and thus, by extension, that there were no reciprocal moral obligations between the races. Buckner Payne, the virulently racist author of the 1867 screed, *The Negro: What Is his Ethnological Status?*, insists that his argument is “purely ethnological and Biblical”, and has nothing to do with slavery (Payne 1867: 3). Whether this is the case or not, one cannot help but notice the distinctly American inflection of his version of pre-Adamism, which is concerned to argue for separate creations of ‘blacks’ and ‘whites’, rather than to account for the origins of New World natives, which had been the central preoccupation of seventeenth-century polygenesis. “It will be admitted by all, and contradicted by none”, Payne proclaims, “that we now have existing on earth, two races of men, the *white* and the *black*” (Payne 1867: 4). Payne believes that blackness does not result from the curse of Ham (see Goldenberg 2003), since nowhere in scripture is it indicated that this curse involved a blackening of the skin, kinking of the hair, and so on, while, manifestly, other curses, such as that of Adam and Eve themselves, did not result in these effects. Payne (1867: 7) concludes that the ‘negro’ “must have been in the ark ... and if he entered it ... he must have *existed* before the flood, and that, too, just such negro as we have now, and consequently not as a descendant of Adam and Eve; and if not the progeny of Adam and Eve, that he is inevitably a beast”.

Polygenesis theory’s nineteenth-century expression bears little relation, as we will see, to the best known early modern version of it, articulated some two hundred years earlier by Isaac La Peyrère, a French theologian and the author of the scandalous 1655 work, *Prae-Adamitae*. There is, for one thing, no suggestion in La Peyrère that not descending from Adam is tantamount to animality. Instead, La Peyrère posits the separate creation principally because he is intent to establish an alternative chronology, which requires that the chronologies related by non-Adamites be taken seriously. According to the early modern iteration of polygenesis, there is in fact a scriptural basis for the view that there were ‘men before Adam’. Properly understood, scripture provides a way of accounting for the presence of human beings at the far corners of the globe, who have no knowledge of the revealed truth of the Judeo-Christian tradition, and with physical appearances that could, with the limited and distorted information available, reasonably be thought to be different enough from those of Europeans to justify the claim of separate creations.

While based in scripture, pre-Adamism undermined one of the central projects of the institutions that derived their authority from scripture: the missionary enterprise. Missions, however dismissive of non-Christian belief systems, were nonetheless based on a presumption of a universal human nature: these people warrant conversion because they are, at bottom, like us. But pre-Adamism was sceptical of this presumption. Given the available information about ancient history and prehistoric migrations of peoples, there was very good reason to think that the inhabitants of different continents and of far-flung islands had to have come into being separately from those people thought to be descended from Adam and Eve. The principle of parsimony spoke against the project of rooting all newly discovered peoples in the available historical tradition, but in the end it was just this that monogenetic accounts of human origins had to do in order to account for the ancestral link of, say, the Native Americans to the first parents in the Garden of Eden.

One of the implicit messages of early modern polygenesis might be summed up as: "Let us leave them alone, for they are not like us". This went directly against the missionary project, which might in turn be summed up as: "Let us interfere in their world, for they *are* like us". Yet by the nineteenth century, polygenesis theory would be remoulded to support a very different sort of exhortation: "Let us dominate them, for they are not like us". In this period, it would come to serve as one of the principle arguments, especially in the United States, in defence of the now threatened institution of slavery. For most in the seventeenth century, it was presumed that because it is the man of the Bible who is created in the image of God, if people on the other side of the world had a separate creation, then they could not but be seen as unequal, in terms of relative likeness to God, to those in the Christian world. And thus monogenesis ensured both the appropriateness of missionary work at all corners of the globe, as well, at least from the point of view of the missionaries, as the full equality – again, in terms of relative proximity to God – of all ethnic groups. In the seventeenth century, to deny the shared origins and equality of all ethnic groups was to deny the universality of scripture and to deprive the project of proselytization of any sense, and was thus deeply heretical.

How, now, do we get from La Peyrère's version of pre-Adamism to that of Payne? I propose, to begin, that the theoretical difference between La Peyrère and Payne – quite apart from any consideration of the social and historical exigencies that made the respective theories attractive – has to do with two fundamentally distinct, but related, ways of accounting for the unity and the distinctness of different ethnic or 'racial' groups. I will refer to these as 'autochthony' and 'traduction'. The distinction might also be captured by that familiar and infamous phrase, 'blood and soil', though by this we must not understand, as the Nazi ideologue Richard Darré intended in coining the phrase, a pair of notions that must go

together. Rather, we must understand two fundamentally different, if compatible, kinds of explanation of what it is that gives a human population its cohesion. The former notion says that it is by descent from common parents, by what Kant would later call 'the unity of the generative principle.' The latter notion identifies shared membership in a discrete population as stemming from a connection to a certain territory of the earth. In its most extreme formulation, the 'blood' principle would have it that no member of a given subpopulation of human beings can share blood or hybridize with a member of another population, either because of an absence of interfertility or, more commonly, because any resulting offspring would belong to the population of neither of the parents. In its most extreme formulation, the 'soil' principle would have it that a given group literally springs forth from the earth in the place where it will continue to reside until the end of days.

I propose, further, that the overwhelmingly more appealing early modern account of human diffusion throughout the globe was traductionist, and that this was coupled with a 'degenerationist' account of human diversity, according to which, over time, climates and environments change the physical appearance of the human populations that inhabit them, even though no amount of change in appearance could ever lead to change of species. I propose, finally, that in the one prominent instance of non-traductionist explanation of human origins in the seventeenth century, pre-Adamism, the motivation for this was not at all a preference for autochthony, but rather a concern to establish the legitimacy of parallel cultural traditions, particularly in the sciences of chronology and astronomy, that are not mentioned in Judeo-Christian scripture. The motivation was not to marginalize non-European peoples as earthborn (and thus soulless) or as extra-scriptural (and thus in an important sense beyond the pale of history), but rather to insist on the legitimacy of cultural traditions that do not trace their own origins back to Genesis. It was not part of the scope of early modern pre-Adamism to offer a typology of separately created human races; where typology was the central concern of an author – as for example François Bernier – there is simply no account of, or apparent interest in, the causes of the apparent physical differences between the races.

There was, in sum, a tremendous controversy in the seventeenth century over the origins of non-European peoples, but the controversy in its essence had to do with the question of the limits of scriptural authority in the context of a newly globalized natural science, *not* with the question of the equality or inequality of the various 'races'.

I would like to proceed to make the case for this cluster of proposals by focusing, principally, on the work of two pairs of seventeenth-century authors – first, Isaac La Peyrère and Matthew Hale; and second, François Bernier and G. W. Leibniz – who were at the centre of the early modern controversies

concerning the origins and diversity of the human species. In each case, we see a sharp critic responding to an influential new theory. Hale responds to La Peyrère's theory of separate creations, while Leibniz responds to Bernier's attempt to offer a typology of the basic 'races' or 'species' of human beings. Taken together, these two related controversies show, first of all, that where ancestral links between human populations were denied, what was at issue was not a separation of 'races' in the later sense that would emerge over the course of the eighteenth century; and where an enumeration of the races was at issue, there was no interest in the question of ancestral links or their absence. In both cases, we find deep disagreements, but what is perhaps more revealing is what the rival parties in the controversies do not so much as think to disagree upon. La Peyrère and Hale both agree that what is in question is the problem of accounting for global diffusion and for alternative historical and scientific traditions, not of accounting for 'racial' difference. Neither Bernier nor Leibniz, in turn, takes an interest in applying the new theory of racial classification to a hierarchy of lower and higher racial types; nor does Leibniz, contrary to a widely held opinion, believe that racial classification reflects any real differences between human groups, which he sees as fully and equally human in virtue of their shared ancestry.

2. Early modern polygenesis theory

While the theological debates that gave rise to pre-Adamism were very old, in the early modern period they were being played out in a new, globalized context. It was above all the discovery of the New World, and the problem of accounting for its inhabitation, that provided a strong incentive for new articulations of this theory. On any non-polygenetic account of the dispersion of the human species, it became difficult to think of human beings as essentially connected to any particular territory or other. The human species came to appear essentially migratory, and any features a group may have for now are only the reflection of their current geographical setting. It was in this respect, among others, that it became easier for many to think of the New World inhabitants as having a separate creation. Thus Francis Bacon writes in his "Of the Vicissitude of Things": "If you consider well, of the people of the West Indies, it is very probable that they are a newer, or a younger people, than the people of the Old World" (cited in Popkin 1987: 39). Indeed, the earliest theoretical elaborations of the polygenesis theory in the age of discovery were put forth within the context of controversies that were being played out in Spanish America in the sixteenth century. While libertines who remained in the Old World, such as Lucilio Vanini and Giordano Bruno, and natural philosophers such as Paracelsus and Andrea Cesalpino were ready to

entertain the idea that barbarians are, like insects, 'imperfect' and thus generated spontaneously out of the earth, without exception the Spanish missionaries who came into contact with Native Americans were intent on arguing for the common descent of all humanity.

As Anthony Pagden (1982) has argued, for the Spanish in the New World, it was the Greek model of the barbarian, translated into the Christian context of an open-ended *congregatio fidelium* that prevailed. For the Greeks, a barbarian was defined simply by the absence of real speech, which in turn was identified with *logos* or rationality, the thing that makes a human being properly human. But this absence was not a genetic condition, and could be corrected even in the course of a barbarian's life. The very goal of the missionaries was to provide such a 'correction.' Thus while Gonzalo Fernández de Oviedo, in his *Historia general y natural de las Indias* of 1535 (Oviedo y Valdes 1535), characterizes the Native Americans as among the most barbaric people on earth, and while Bartolomé de Las Casas for his part, in his *Apologética historia* (1967 [1551]), denounces the Europeans as the true barbarians in view of their brutality towards the Americans, both of these diametrically opposed authors agree that Europeans and Americans had to have had one and the same creation. Las Casas argued, instead, that it is Africans who are truly subhuman, and who should thus be imported to the New World *en masse* for slave labour, even as the Native Americans he defended were being converted to, and purportedly saved by, Christianity.

In these early debates, physical appearance does not appear to have played a significant role in the appraisal of different groups of people as either intrinsically or accidentally barbarian. Rather, the case was made on the basis of the perception of cultural difference. Later, in the second half of the seventeenth century, when polygenesis became a viable position to take up in the controversy over human diversity (as opposed to a mere libertine provocation), pre-occupation with 'racial' difference still remained more or less absent. What was of greatest concern now was not so much accounting for either physical difference or perceived cultural inferiority, but rather accounting for (i) the widespread diffusion of human beings throughout the world; (ii) the fact that many of these human beings had alternative chronologies that contradicted the one familiar from Genesis; and (iii) the apparently extra-historical (because extra-scriptural) status of the various widely dispersed groups of people. Until the age of exploration, 'history' was understood as encompassing only those groups of people for whom there was already an available textual tradition; the only way to make newly discovered or far-flung peoples historical was to connect them to these available traditions. Thus in an earlier work, the 1644 *Relation de l'Islande*, La Peyrère (1644: §35) criticizes the view that the Icelanders, along with their revered 'prince' Odin, are descended from an errant faction of the

ancient Roman army as follows: “[W]hat is the hope of being able to accept all of the fables that they tell about this Asiatic Odin, and what connection could such weak fables have with the age of Pompeius, which is an age so well known, and so historical?” Here, the pre-Adamite author is suggesting that it is much easier to suppose that far-flung peoples – and here, as also in Grotius, Iceland is thought to be a sort of stepping-stone towards the New World, rather than an island securely within the European sphere – have their own histories, whether written or unwritten, rather than to strain to derive them from the available textual traditions beginning in the Mediterranean region.

La Peyrère’s eventual defence of pre-Adamism would be motivated by just this sort of questioning. In his *Prae-Adamitae*, which appeared in English translation in 1656 under the title, *Men Before Adam*, La Peyrère cites Romans 5: 12–14 as support for the Pre-Adamite hypothesis. Here the apostle Paul writes that before “the time of Law sin was in the world”, and La Peyrère interprets this to mean that there were sinful *people* already when, with Adam, law came into the world. Paul’s epistle served to include all newly discovered peoples within the core textual tradition of Europe, even as it excluded them from the group of people who were bound by ancestry to this tradition. It made sense of them, insofar as it found a mention of them in the Bible; but at the same time it denied that the Bible could have any authority for them.

The author was pressured into retracting the views defended in this work, but not soon enough to prevent his argument from making a profound impact. There were at least a dozen important treatises in the latter half of the seventeenth century seeking to refute La Peyrère’s thesis. Three of these were of particular significance, all published in 1656: Philippe Le Prieur’s *Animadversiones in librum Prae-Adamitarum*, Johann Hilpert’s *Disquisitio de Prae-Adamitis* and the revealingly titled work of Anton Hulsius, *Non ens prae-adamiticum, sive confutatio vani et socinizantis cujusdam somnii, quo S. Scripturae praetextu in cautioribus nuper imponere conatus est anonimus fingens ante Adamum primum homines fuisse in mundo*, which is to say: “No pre-Adamite being, or, a confutation of a certain someone’s vain and Socinianizing dream, by which an anonymous author, on the pretext of the Holy Scripture, endeavored not long ago to establish to the incautious that there were men in the world before the first Adam” (Hulsius 1656). There were, in fact, far more refutations than defences, and every defence was shrouded in either caution or anonymity. Richard Popkin (1987: 115) explains that “[p]ractically nobody in the seventeenth century was willing, publicly, to accept the pre-Adamite theory or any form of polygenesis. The irreligious implications were too great for the theory to be given much credence prior to the Enlightenment ... The explanatory value of a polygenetic theory was great, but the danger of holding to it was, perhaps, greater”.

La Peyrère's thesis was however at least cautiously praised by a number of prominent figures in the Republic of Letters, including Guy Patin and Marin Mersenne, both of whom saw in it a promising way of making sense of certain problems of biblical exegesis, not only with respect to Paul's epistle to the Romans, but also the more well-known problem of accounting for the origins of Cain's wife in the book of Genesis (Gliozzi 2000: 445–446). But Patin and Mersenne expressed their opinions in private correspondence, and only on the basis of their knowledge of the pre-circulated manuscript of La Peyrère's *Prae-Adamitae*. After it was published in 1655, enormous controversy ensued. La Peyrère had set off a war with many fronts. His thesis was challenged on the grounds that it did not in fact adequately account for events described in scripture. From the other direction, it was challenged on the grounds that there are other, less radical ways of accounting for both human diffusion throughout the globe and the alternative chronological traditions of non-European peoples.

Some authors, particularly liberal Jesuits who were deeply familiar with non-European scientific traditions, would, shortly after the initial round of attacks on La Peyrère, begin expressing cautious sympathy for polygenesis on the grounds that no plausible reading of Genesis can possibly accommodate the vastly older calendrical systems of, e.g., the Chinese and the Mexica. As William Poole (2004) notes, much of the evidence for alternative datings of the earth's distant past was being accumulated and reflected upon by Jesuit missionaries who considered it part of their missionary task to learn as much as they could about the scientific accomplishments of the peoples they sought to convert. It was the Jesuit Martino Martini's *Sinicae Historiae Decas Prima* (Martini 1658) that called attention to Chinese chronology's incompatibility with that of the Old Testament, and on this basis explicitly questioned Biblical universality. For Martini as for other cautious sympathizers, it was chronology rather than phenotypic variety that presented the strongest argument in favour of pre-Adamism and against monogenesis.

The pre-Adamite thesis was challenged at once on the grounds of biblical exegesis, the implausibility of extra-scriptural chronologies, and the plausibility of traductionist accounts of the population of the earth. Accounting for the details of global traduction was a task that required the assembly and interpretation of extremely fragmentary geographical and linguistic data. Some of the speculative proposals from the mid seventeenth century as to the possible ancestry of the Native Americans appear outlandish, but without exception they also reveal the lack of any perception of 'racial' difference between the New World inhabitants and 'white' Europeans. Already in 1642, Hugo Grotius conjectured, in his *De origine gentium americanarum* (Grotius 1642) that the Americans are of Norwegian origin, since it is only the Germanic languages that have any clear similarity with the languages of the New World. Grotius arrives at this position, as the least im-

plausible one, by a process of elimination: the only alternative to the hypothesis of Scandinavian origins, he reasons, would be to hold “either that they existed from eternity, according to the opinion of Aristotle; or that they were born of the earth, as a fable tells us concerning the Spartans; or from the ocean, as Homer maintains; or indeed that they were created before Adam, as someone in France imagined recently”. All of these views, Grotius adds, “seem very dangerous for piety, while believing what I have said is not so at all” (Grotius 1542: 15; cited in Gliozzi 2000: 450). Only diffusion or traduction offers a way of accounting for human diversity while avoiding impiety.

In sum, while La Peyrère’s account of human diversity in terms of separate creations was fiercely contested, and cautiously defended, the controversy centred around the proper interpretation of scripture, the relevance of alternative scientific traditions, and the problem of diffusion. It did not have anything to do with ‘race’ in the way this would come to be understood over a century later. The problems of diffusion and chronology, as opposed to ‘racial’ difference, are dealt with at length by the English jurist Matthew Hale in his work *The Primitive Origination of Mankind* (1677). Hale’s contribution to the early modern polygenesis debate is of particular interest in view of its treatment of the problem of human diversity as a problem of natural philosophy, which is to say of science, rather than of biblical interpretation. Let us look at his contribution to the debate in some detail now.

3. Hale’s bio-geographical account of human diversity

Interestingly, while La Peyrère accepts the heretical doctrine of polygenesis, and Hale rejects it, it is the heretic who spends almost all of his effort explicating scripture and looking for a justification of his view in the holy writ, while the traditionalist for his part devotes much of his energy in the *Primitive Origination* to providing a plausible naturalistic account of diffusion into, and biodiversity in, the New World.

Hale explicitly identifies the possibility of the spontaneous generation of insects as relevant to our understanding of the ‘primitive origination of mankind’. He seeks to provide an account of how it is that human beings could have arisen *ex non genitis*, that is, from elements or principles that were not themselves generated. He maintains that this production could have happened in one of three ways: it could have been “fortuitous or casual”, it could have been “natural”, or it could have happened “by the immediate Power, Wisdom, and Providence of Almighty God and his meer *Beneplacitum*” (Hale 1677: 256). All of these three ways, in any case, would require that the first creatures of any species came into being in a different way than all of their subsequent descendents; they would have

to be produced, namely, *ex non genitis*, or from things that are not themselves generated, whether these be atoms or God. Hale believes that this “Method of production of Men and perfect Animals is ceased” in the present age, “and their production now delegated ordinarily to Propagation”, but he considers the possibility that “in some places, and at some times, especially between the *Tropicks*, such a Pullulation of Men and Beasts may be supposed to be” (Hale 1677:257). Hale rejects this possibility, but his characterization of the view of his opponents is significant, since it shows the widespread association in the early modern period (an association extending back to antiquity) between those parts of the world inhabited by Africans, and later also by New World natives, on the one hand, and the possibility of being born from the earth on the other.

In fact, though, for Hale, spontaneous generation of any sort is excluded. “[E]ven the generality of Insects, which seem to be spontaneous, is truly seminal and univocal”. Why does Hale want to foreclose the possibility of spontaneous generation even of the ‘imperfect’ or ‘indeterminate’ creatures? For him, imperfect insects “were at first in their first Creation determinate and certain; and although since partly by degeneration, partly by various mixtures their *Species* are changed and multiplied, even as the perfect Animals in *Africa* are by a mixture of *Species*, yet they were at first determinate”. He reasons that insects are “in some respect more admirable” than larger animals, “as a small Watch is an evidence of greater skill and artifice than a greater”. For this reason, he refuses to “fetch Arguments against the like spontaneous Productions of the greater Animals from any contemptible valuation of these smaller, and these little Models of sensible Life” (Hale 1677:272–273, 276).

According to Hale, the first generation of creatures was by “divine power and ordination”, and not from any pre-existing *primordium* or seed. But once the divinely ordained kinds were in existence, all future generation was by way of ‘propagation’, which is to say by way of sexual reproduction. “[T]hat Prolifick Power of propagating”, he explains, “was never delegated or committed to the Earth, or any other Casual or Natural Cause; but only to the Seminal Nature, derived from their Individuals, and disposed according to that Law of propagation of their kind, alligated as before to their specifical and individual nature” (Hale 1677:305). Should we suppose any other kind of origination of humanity than by God’s divine creation, “we may with as fair a Supposition imagine that a Man should be produced by the natural conjunction of Sheep or of Lions, or a Star be produced *ex putri materia terrestri*, as to suppose a Man to be produced accidentally, casually, or naturally” (Hale 1677:316). In short, generation by fortuitous causes is just as much a disruption of the cycle of ordinary species reproduction as would be the monstrous birth of a creature of one species from parents of another. Hale finds far more reasonable the “divine hypothesis” that “the first Individuals of Humane

Nature, had their Original from a Great, Powerful, Wise, Intelligent Being" (Hale 1677: 305f.).

But why not hold open the possibility that there had been several independent 'originals' of this sort for different human populations? Earlier, in Section II of the *Primitive Origination*, Hale notes that

[t]he late Discovery of the vast Continent of *America* and Islands adjacent, which appears to be as populous with Men, and as well stored with Cattel almost as any part of *Europe*, *Asia*, or *Africa*, hath occasioned some difficulty and dispute touching the Traduction [note the term] of all Mankind from the two common Parents supposed of all Mankind, namely *Adam* and *Eve*. (Hale 1677: 182)

The greatest problems arise, Hale believes, "concerning the storing of the World with Men and Cattel from those that the Sacred History tells us were preserved in the Ark" (Hale 1677: 182). He sharply recognizes that the study of 'Cattel' can help us to learn about the diffusion of men, and his proposal for studying the human past in part by looking at current biodiversity is remarkably advanced. There are, he notes, "divers perfect Animals of divers kinds in *America* which have none of the same kind in *Europe*, *Asia* or *Africa*". From this, "many people conclude that the Americans could not be descended from Adam" (Hale 1677: 182f.). He summarizes their position as follows:

That since by all Circumstances it is apparent that *America* hath been very long inhabited, and possibly as long as any other Continent in the World, and since it is of all hands agreed that the supposed common Parents of the rest of Mankind, *Adam*, *Noah* and his three Sons, had their Habitations in some Parts of *Asia*, and since we have no probable evidence that any of their Descendents traduced the first Colonies of the *American* Plantations into *America*, being so divided from the rest of the World, the access thither so difficult, and Navigation the only means of such a Migration being of a far later perfection than what could answer such a Population of so great a Continent: That consequently the *Americans* derive not their Original either from *Adam*, or at least not from *Noah*; but either had an Eternal Succession, or if they had a Beginning, they were *Aborigines*, and multiplied from other common Stocks than what the *Mosaical* History imports. (Hale 1677: 183)

Hale sharply disagrees, though he does acknowledge some ethnographic evidence that supports the polygenetic conclusion. Though the traditions of America "be mingled with some things fabulous", nonetheless "they seem to favour [the] Conclusion" of the polygenesis theorists (Hale 1677: 183). Yet Hale's preferred sort of evidence is not ethnographic or scriptural, but rather bio-geographical. He argues that "the Origination of the common Parents of Mankind were in *Asia*, yet some of their Descendents did come into *America*". He suggests that the

“transmigration” from Asia to America could have happened by land or by sea, but that the latter is more likely, since, “though it may be possible that there may be some junctures between the North Continent of *America* and some part of *Tartary*, *Russia*, or *Muscovy*, yet none are known, unless the Frozen Seas in those Parts might be a means to transport Men thither” (Hale 1677: 189). (It is interesting to note in passing that one of the principal motivations Leibniz would have in gaining influence with the tsar of Russia would be to convince him to organize an expedition to determine whether Asia and America do in fact connect: a plan that would eventually be realized in Vitus Bering’s second Kamchatka expedition of 1733–1743). In a remarkable illustration of the absence of any perception of what we would think of as racial difference, Hale suggests that “those Countries in *Asia* or *Europe* that with greatest probability first peopled *America*, seem to be”, in the following order: (i) the ancient British; (ii) the Norwegians; (iii) the Tartars or Scythians; (iv) the Phoenicians or Carthaginians; or (v) the Chinese (Hale 1677: 195). Any one of these groups, in Hale’s view, could have given rise to the current population of the Americas.

Hale also reasons that the current distribution and shape of land masses in the world is not the same as it once was. The prospect of continental drift and erosion had been widely entertained at least since Abraham Ortelius’s *Thesaurus geographicus* (1587), though as far as I know Hale is among the first to use it to support an argument in favour of a diffusionist account of the unity of the human species. He argues at great length that “we can by no means reasonably suppose the Face, Figure, Position and Disposition of the Sea and dry Land to be the same anciently as now”, and that “those parts of *Asia* and *America* which are now disjoined by the interfluency of the Sea, might have been formerly in some Age of the World contiguous to each other” (Hale 1677: 193).

But what about animals? Hale notes that, even if we suppose the human beings got there *ex industria*, still “it is not easily conceivable how Beasts, especially of prey, should be transported into *America* through those large Seas” (Hale 1677: 184). But he is not easily dissuaded from his diffusionist convictions, and he goes on to argue, on the basis of his reading of “Grotius, Laetius, Breerwood, Hornius, Josephus Acosta, Mr. John Webb, Martinius and others”, that there is a plausible way of accounting for the “Manner of Traduction of Brutes into *America*” (Hale 1677: 189). The variety of flora and fauna in the New World that are different from those of the Old World can be explained, Hale thinks, along degenerationist lines, by “the promiscuous couplings of Males and Females of several *Species*, whereby there arise a sort of Brutes that were not in the first Creation” (Hale 1677: 199). Though any species remains one and the same thing throughout its history, in view of the ‘unity of the generative power’ (to borrow Kant’s phrase again), nonetheless Hale’s degenerationism allows for intra-species variation

resulting from environmental factors. After all, he notes, “among Animals, the *Indian* Elephants are larger than the *African* [in fact it is the other way around]; the *English* Mastiff degenerates in his courage and fierceness, at least in the first succession by generation, when brought into *France*”. Hale suggests that the Llamas of Peru might “be primitively sheep” (Hale 1677:201). As with animals, so too with the variety of humans:

Nay let us look upon Men in several Climates, though in the same Continent, we shall see a strange variety among them in Colour, Figure, Stature, Complexion, Humor, and all arising from the difference of the Climate, though the Continent be but one, as to point of Access and mutual Intercourse and possibility of Inter-migrations: The *Ethiopian* black, flat-nosed and crisp-haired, the *Moors* tawny; the *Spaniards* swarthy, little, haughty, deliberate, the *French* spritely, sudden; the Northern people large, fair-complexioned, strong, sinewy, courageous ... And there is no less difference in the Humors and Dispositions of People inhabiting several Climates, than there is in their Statures and Complexions. And it is an evidence that this ariseth from the Climate, because long continuance in these various Climates assimilate those that are of a Foreign extraction to the Complexions and Constitutions of the Natives after the succession of a few Generations.
(Hale 1677:200–201)

In effect, Hale derives the entire human species from traduction, and in order to do this he makes highly speculative conjectures about the migrations of peoples out of the Near East, and also about the way in which environments can change appearances without changing fundamental relations of kinship. For him, blood is key, and any influence of soil (along with sun, atmosphere, and so on), is merely superficial. Hale is interested in differences in the ‘constitutions’ of human groups, but does not believe that these differences should be explained in terms of any basic racial classificatory schema. He is concerned with ‘national physiognomy’, not the typology of races. According to many scholars, the first such typology would be presented only in the decade following Hales *Primitive Origination*, by the seventeenth-century French libertine philosopher and intrepid voyager François Bernier.

4. François Bernier’s “New Division of the Earth”

Bernier is the first author to use the term ‘race’ to designate different groups of humans with shared, distinguishing traits. He describes his innovation in an article titled, “A New Division of the Earth, by the Different Species or Races of Man”, published in the *Journal des Sçavans* in 1684. “So far”, he writes, “Geographers did not use any other criterion when mapping out the earth but that of the different

countries or regions to be found on it. What I noticed in men in the course of my long and frequent travels gave me the idea to divide the Earth otherwise" (1684: 133).

According to Siep Stuurman (2000: 1), Bernier's "Nouvelle division" is in some respects "a typically seventeenth-century anthropological essay; in other respects it anticipates the eighteenth-century genre of the natural history of mankind". Is this a correct assessment? By his own lights, Bernier's work is novel in that it does not simply divide populations up according to what might be called 'national physiognomy', which is a common practice going back at least to Hippocrates. Instead, he tries to find a small number of more basic classifications. "For although in the exterior form of the body", he explains, "and particularly of the face, men are almost all different from one another, depending upon the cantons of the earth in which they live, so that those who have travelled a great deal can often distinguish in this way, without mistake, each nation in particular, I have nevertheless remarked that there are above all four or five Species or Races of men, whose difference is so great that it could serve as a good foundation for a new division of the world" (Bernier 1684: 133–134).

Curiously, Bernier includes within one and the same race all of the people of Europe, together with all of the peoples of the places in which he has travelled most extensively, particularly Persia and "the states of the great Moghul", as well as the people of Siam, Sumatra, Bantan, and Borneo. Bernier believes that, although the people in some of these regions have a very different skin color than do Europeans, "this color is however only accidental to them, and occurs only because they are exposed to the sun". Those among them who are not required to expose themselves to the sun "are not much more black than many Spaniards". Bernier concedes that many Indians "have something quite different from us in the conformation of the face, and in their color, which approaches to yellow". But he says that if this is enough "to make of them a particular species", then "it would be necessary to make one of the Spanish as well, another of the Germans, and similarly for some other peoples of Europe" (Bernier 1684: 134–135). Clearly, even if there was a perception of physical differences between national groups, they did not at all match many of the stereotypes that would emerge by the nineteenth century (in particular, the core doctrine of Romantic 'Aryanism', which takes Germans as the representatives *par excellence* of the European racial type).

Bernier goes on to claim that the peoples of "all of Africa, besides the coasts" are characterized by the fact that, unlike the "accidentally black" Indians, "for them blackness is essential". He avers that "it is necessary to seek the cause [of the blackness of the skin] in the particular contexture of their bodies, or in the semen, or in the blood, which is nevertheless of the same color as everywhere else". He also makes fine-grained distinctions that would be lost on later racial science; for

example, he notes that “the Blacks of the Cape of Good Hope” seem to be “of a different species than the rest of Africa” (Bernier 1684: 135).

The third ‘species’ for Bernier are ‘Asians’, which includes for him the inhabitants of “part of the kingdoms of Aracan and Siam, Sumatra and Borneo, the Philippines, Japan, China, Georgia and Muscovy, the Usbek, Turkistan, Zaquetay, a small part of Muscovy”. Finally the fourth species are not, as one might expect, Native Americans, but rather the Saami or Lapps, about whom he writes they are “very ugly and partaking much of the bear”. He acknowledges: “I have only seen two of them at Dantzic; but, judging from the pictures I have seen, and the account which I have received of them from many persons who have been in the country, they are wretched animals”. The ranking of Lapps at the bottom of the scale of humanity would remain a commonplace throughout the eighteenth century, in Buffon, Maupertuis, Kant, and others. As for Native Americans, Bernier maintains that “most of them have olive skin and a face that is differently shaped than ours, nevertheless I do not find there sufficient difference for making of them a particular species that is different from ours” (Bernier 1684: 136).

Taken together, the facts that (i) Bernier dismisses Lapps as “wretched animals”; (ii) identifies Africans as “essentially black” while consistently avoiding any value-laden language as to where this essential difference places them relative to Europeans; (iii) places many of the inhabitants of the Indian Ocean rim in the same ‘race’ as Europeans, while insisting that if, for example, Sumatrans are to be excluded from this race, then so are Germans; and, (iv) finally, places Native Americans in the same race as Europeans: all of this suggests that Bernier had a very different range of interests and concerns than later proponents of racial science. For one thing, he was implicated neither in the project of dominating the New World, nor in the project of trans-Atlantic slavery. Perhaps as a consequence, he does not dwell on the supposed racial differences of Europeans, Africans, and Native Americans. What interests him far more is Eurasia, and here his divisions end up looking nothing like the separation of the ‘Mongoloids’ from the ‘Caucasoids’ that would become so familiar after the publication of Blumenbach’s *De generis humani varietate nativa* in 1776 (Blumenbach 1865).

Anyone who wishes to characterize Bernier as an early racial theorist should bear in mind that the last four pages of his very concise, seven-page article are devoted to extolling the beauties of the women of various nations of the world. Bernier is convinced by his extensive travels that “beautiful and ugly [women] are everywhere to be found”. He is particularly fond of women of the second species, and recalls seeing several of them in Ethiopia “who were for sale, and I can say that one can see nothing more beautiful in the whole world; but they were extremely expensive, for they were being sold at three times the price of the others” (Bernier 1684: 137). This is curious material, but it is clearly more in the vein of boastful

travel literature than racial science. After his description of the southern Africans differences from their neighbours, there is no further account of any physical differences between different groups, let alone any account of their causes.

There is rather more evidence for Bernier's considered views of the nature of human diversity in works other than the "Nouvelle division". In an important treatise on the peoples, customs, politics, history, and resources of South Asia, translated into English and published as *The History of the Late Revolution of the Empire of the Great Mogol* in 1671, the French traveller reflects on the work of an Indian poet who describes the 'paradise' of 'Indostan' at the confluence of four rivers: "the River *Ganges* on the one side, that of *Indus* on the other, the *Chenau* on a third, and the *Gemma* on the fourth". Bernier notes that if the poet "had concluded, that this was certainly the true Terrestrial Paradise, rather than that in *Armenia*, this would have much enhaunced the worth of all his Commendations" (Bernier 1684: 93). In effect, Bernier here regrets that the authors of the Indian literary tradition do not explicitly lay claim to one of the most important places in scriptural geography. If they had done so, it would have been plausible to grant it to them, given the actual topographical features of northern India, and given the relative closeness of this part of the world to the favoured candidate in the South Caucasus. But without such an explicit identification India lies beyond the pale of history; it does not identify itself with the texts that lie at the origin of everything that could in the seventeenth century be recognized as falling within the scope of history. This extra-historicity of many of the earth's peoples will be, as we will later see, one of the principal motivations for early modern polygenesis theory.

In the course of describing Hindu customs of ablution, Bernier has occasion to make a revealing observation about the question of the universality and locality of religious traditions: "When I told them", he writes,

that in cold Countries it would not be possible to observe that Law of theirs in Winter (which was a sign of its being a meer human invention) they gave this pleasant answer: That they pretended not their Law was universal; that God had only made it for them, and it was therefore that they could not receive a Stranger into their Religion: that they thought not our Religion was therefore false, but that it might be it was good for us, and that God might have appointed several differing ways to go to Heaven; but they will not hear that our Religion should be the general Religion for the whole earth; and theirs a fable and pure device.

(Bernier 1684: 149–150)

From the seventeenth-century European point of view, it is but one small step from supposing that God appointed several ways of going to heaven through several religions to supposing that God separately created several groups of people, each with its own religion. After all, what it is to have the religion of the Judeo-Christian tradition just was to suppose of oneself that one was descended from

Adam and Eve. The only conceivable way for a person to fall outside of the scope of this tradition was to come from a separate line of descent. Interfaith respect – which Bernier disdains – practically requires a commitment to polygenesis.

While unwilling to accept polygenesis, it is important to remember that Bernier was at least a moderate libertine, as well as a devoted disciple of the materialist philosopher Pierre Gassendi, and even published a textbook summary of the philosophy of the latter, the *Abregé de la philosophie de Mr. Gassendi*, in 1678 (Bernier 1678). Interestingly, Gassendi himself makes a revealing comment, if only in passing, about the variety of the human species and about the problem of determining the boundaries of the species. In his *Mirror of True Nobility* (1657), published in English translation in 1657, Gassendi describes a relation of a certain Thomas Arcosius to Nicole-Claude Fabri de Peiresc, in which he related

what had happened to one of *Ferrara*, when he was in a Country of *Marmarica*, called *Angela* [i.e., Angola]. For he happened one day upon a Negro, who hunted with Dogs certain wild men, as it seemed. One of which being taken and killed, he blamed the Negro for being so cruel to his own kind. To which he answered, you are deceived; for this is no man, but a Beast very like a man. For he lives only upon Grasse and has guts and entrals like a Sheep, which that you may believe, you shall see with your eyes; whereupon he opened up his belly. The day following, he went to hunting again, and caught a male and a female. The female had Dugs a foot long; in all other things very like a Woman ... Both their Bodies were hairy all over, but the hair was short and soft enough.

(Gassendi 1657, Book 5:92–93)

Now of course this anecdote is mediated through several parties by the time it reaches Gassendi, yet it reveals something significant about the way variety within the human species was understood by Gassendi's contemporaries, a way that Gassendi himself transmits without opposition: there is a fact of the matter as to who is a human being and who is not, even if there is great variety in physical appearance. If beasts, such as those hunted by the "Negro" in this tale, happen to have humanoid features, there is nonetheless a way of determining absolutely whether they are in fact human or not; in this case, cutting them open and looking at internal anatomy suffices. If a creature is not 'of one's own kind', but only a beast, then there is no moral concern about cruelty towards it. But while the European supposes that the kind in question here is "Negro", the "Negro" in turn denies that the beast belongs to the *human* kind: he does not deny that it is "very like" a "Negro", but that it is "very like a man". The European, and eventually Gassendi as well, does not find anything wrong with this shift, and for this reason, presumably, when 'kind' is used here in reference to 'negroes', this does not isolate this kind in any natural or essential way from the human kind of which it is a part.

By ‘species’, or by its synonym ‘race’, Bernier could not have intended the meaning commonly attached to this term today – namely, that each race is an isolated reproductive group – any more than Gassendi intended this when he spoke of the Negro ‘kind’. Though Bernier himself was not a defender of the doctrine, some of his contemporaries would come to hold the view that different races constitute different ‘species’ in the sense that, while capable of yielding offspring, they nonetheless had separate creations and, therefore, arose from separate lines of descent.

In my view, Bernier’s role in the emergence of the modern conception of race has been somewhat overstated: his “new division of the earth” is principally motivated by questions of physical geography (as well as simple story-telling), and he is more or less silent as to the deep nature of the difference in appearance and character of the different groups of people he considers. The historical explanation of this is simple: he happens to have been read by Leibniz, who happens to have been read by Blumenbach, who in turn wrote the *De generis humani varietate nativa*, which served to codify the most basic outlines of racial classification, which would remain in place until the mid twentieth century. Leibniz, for his part, though far from defending any theory of essential differences between different human races, did, as we will see presently, at least offer an explicit account of what a race is.

5. Leibniz: Race as generational series

Leibniz’s name, Peter Fenves writes, “is often found in lists of those who were early proponents of a racial system of human classification”. Fenves notes that “Leibniz concurs with – and gives credence to – the novel representation of supranational distinctions that François Bernier first proposes in his 1684 article” (Fenves 2005:73f.). As Fenves rightly goes on to note, however, while it is clear that Leibniz read this issue of the *Journal des Sçavans*, this reading did not make enough of an impression on him for him to retain Bernier’s name. In a letter of thirteen years later to the groundbreaking Swedish linguist Johan Gabriel Sparvenfelt, Leibniz writes:

If it is true that the Kalmuks as well as the Moguls and Tartars of China depend on the Grand Lama in matters of religion, it is possible that this says something about the relation among their languages and the origin of these peoples. It is simply that the size and constitution of their body is so different among them.

(Leibniz 1718:36–37)

Here, then, Leibniz is maintaining that the physical appearance of members of a population can be misleading, and that language is far more important than 'race' for determining ancestral relations. Nonetheless, he is aware of the interest in 'racial' classification that other authors have. He writes in another, undated letter to Sparvenfelt:

I remember reading somewhere, though I cannot recall where [evidently a reference to Bernier], that a certain voyager divided human beings into certain tribes, races, or classes. He assigned a particular race to the Lapps and Samoyeds, a certain to the Chinese and neighboring peoples; another to the Negroes, still another to the Cafres or Hottentots. (Ibid.)

However, Leibniz abandons the project of a broad taxonomy of the most basic human kinds, and reverts to what we might call 'national physiognomy':

In America there is a marvelous difference between the Galibis or Caribbean, for example, who have a great deal of value and just as much spirit, and those of Paraguay, who seem to be children or youths all their lives. This does not prevent all human beings who inhabit the globe from being of the same race, which has been altered by the different climates, as we see animals and plants changing their nature and becoming better or degenerating. (Ibid.)

Further in the same work in which this letter was published – one of the earliest collections of Leibniz's writings after his death, the *Otium hanoveranum* of 1718 – the editor, Joachim Friedrich Feller, provides another text, whose provenance I have not been able to determine, consisting in a nearly exact Latin paraphrase of the contents of Bernier's "Nouvelle division" (Leibniz 1718: 158–160). The text is interspersed with misspelled Italian expressions, motivating the conjecture that Leibniz composed it during his Italian voyage of 1689–90 for a colleague who could read no French, and amused himself in doing so by trying out his own elementary Italian skills. In any case, it is clear that Leibniz's reasons for writing it fall short of assertion of the truth of its claims. Later, Johann Friedrich Blumenbach would incorporate elements from the *Otium hanoveranum* into his *De generis humani varietate nativa* of 1776, evidently failing to recognize that Leibniz is not stating his own views, but rather summarizing those of Bernier. Blumenbach writes there that "Leibniz divides the men of our continent into four classes. Two extremes, the Laplanders and the Ethiopians; and as many intermediates, one eastern (Mongolian), one western (as the European)" (Blumenbach 1865: 267).

Thus, the identification of Leibniz as a seminal thinker in the history of racial science appears to result from a simple mistaking of indirect reference for direct. For Leibniz, in fact, it is historical linguistics, and not the study of morphological differences that will give us insight into the true lineages of the various human

groups. As he concludes in the text just cited, adding his own view to the long summary of Bernier's system: "I should like for the regions [of the world] to be divided according to languages, and for this to be noted on maps" (Leibniz 1718: 158). For Leibniz, it is neither blood nor soil, but speech, that reveals kinship. His new division of the earth would divide it up according to language families, not phenotypes. Beyond linguistic community, there is for Leibniz no meaningful classification of human populations short of their membership in the human race as a whole. As far as blood is concerned, everyone is related. This commitment was a fundamental principle of both Leibniz's political philosophy and his metaphysics of possible worlds (see Dascal 1993). Popkin maintains that Leibniz did not seem to be interested in the historical, chronological, anthropological questions that provided the ammunition for the pre-Adamite theory (that is, the prevailing version of polygenesis in the seventeenth century, according to which there were separately created human beings living before Adam's creation). Leibniz was, Popkin writes,

very much concerned to discuss other theologies outside of Christianity in terms of their ideological content, but not their differing claims about the facts of human history [...] His efforts to unite the churches within Christendom, and then to unite them with Islam and Chinese religion, did not involve finding common historical ground, but rather common metaphysical and moral ground.

(Popkin 1993: 381)

Yet, *pace* Popkin, we know from scattered passages that Leibniz was a committed monogeneticist, and that he believed that human phenotypic diversity is a consequence of environmental influence over time. Thus he notes in a text from the *Otium hanoveranum*, that all of humanity must belong to the same species, even if "they have been changed by different climates just as we see that animals and plants change their nature, in becoming better or degenerating" (cited in Pagden 1982: 138). For Leibniz, there is only one origin for human beings, and subsequently the boundaries of the human species must remain rigidly fixed. In this respect, Leibniz may be described as a moderate degenerationist, who believes that human diversity can be accounted for in terms of environmental pressures over time in different habitats.

Leibniz was, at least early in his life, an ardent advocate of the first, but did not even have the second available to him to support or to reject. In an audacious text of 1671, entitled "A Method for Instituting a New, Invincible Militia that can Subjugate the Entire Earth, Easily Seize Control over Egypt, or Establish American Colonies", written as an addendum to his better known *Consilium Aegyptiacum*, Leibniz sketches out a plan for training a new army of warrior slaves:

A certain island of Africa, such as Madagascar, shall be selected, and all the inhabitants shall be ordered to leave. Visitors from elsewhere shall be turned away, or in any event it will be decreed that they only be permitted to stay in the harbor for the purpose of obtaining water. To this island slaves captured from all over the barbarian world will be brought, and from all of the wild coastal regions of Africa, Arabia, New Guinea, etc. To this end Ethiopians, Nigritians, Angolans, Caribbeans, Canadians, and Hurons fit the bill, without discrimination. What a lovely bunch of semi-beasts! But so that this mass of men may be shaped in any way desired, it is useful only to take boys up to around the age of twelve.

(Leibniz A, IV, 1, 408; see also Dascal 1993: 391–392)

Leibniz proposes to segregate these prisoners according to language, which for him is the same as segregation by race or genus. In this way, unable to communicate with any warriors beyond their own small squadron, the warriors will be unable to plan an insurrection. “In every race [*genere*]”, Leibniz writes,

whoever is most trained in his squadron, which is to say among those who speak his language, shall challenge those who are the best trained in the other squadrons. The people [*gens*] that wins that year shall be the leaders. They will be able to strike terrible blows with their very powerful curved swords, to hit targets with their slings, and to rip things apart with their lances. They are to be trained to run races at such a speed as will be equal to that of horses. Which will come about first by pursuing them until they are able to touch the mane or the tail, and then freely [i.e., *without horses*] ... They shall learn to jump after the manner of the Tenerifeans, first jumping with the help of a lance ... as far as human strength is able to reach, and afterwards without these. (Leibniz A, IV, 1, 408–409)

Leibniz goes on to describe the tremendous feats these warriors will perform with their lances:

In the beginning they will alight from a higher place by the means of their lance touching the ground below; then they will leap horizontally on a level plane, and finally from below they will leap to the top. They will learn how to climb up smooth surfaces [*per lubrica klettern*] They shall become used to climbing however high their lance may be just by means of fixing their lances beneath them. They will learn moreover to carry the greatest and strongest lances, like Achilles, and like other ancients. Indeed, they shall learn to project them with great impetus towards a designated target, as well as of bringing one lance together with another if the one does not suffice for climbing. By means of this art they will easily conquer the mightiest European fortifications. They will be able to walk on their lances, as on stilts [*wie auff stelzen*]. (Ibid.)

Wherever did Leibniz learn so much about Tenerifean martial arts? In an anonymous travel report in Thomas Sprat's 1667 *History of the Royal Society*, we find the

following description of a native Tenerifean ‘Guanchio’ [today called Guanches], a description that Leibniz would reproduce four years later, sometimes nearly word-for-word:

[An English traveller] himself hath seen [the Guanches] leap from Rock to Rock, from a very prodigious height, till they came to the bottom, sometimes making ten fathom deep at one leap. The manner is thus: First they *Tertiate* their Lance (which is about the bigness of a half Pike) that is, they poise it in their hand, then they aim the point of it at any piece of a Rock, upon which they intend to light (sometimes not half a foot broad.) At their going off they clap their feet close to the Lance and so carry their bodies in the Air. The point of the Lance first comes to the place, which breaks the force of their fall; then they slide gently down by the Stage, and pitch with their feet upon the very place they first designed, and from Rock to Rock till they come to the bottome. Their Novices sometimes break their necks in learning. (Sprat 1667: 212–213)

Knowing this background to Leibniz’s “Method” in Sprat’s *History*, what has been denounced as a grossly racist tract by the young and impetuous Leibniz begins to appear more as the harmless musings of someone who has just read a compelling adventure story – one that happens to be set in the Canary Islands. The choice of settings is not at all coincidental. It is significant that, as historians such as David Abulafia (2008) have noted, the Canary Islands, explored already by 1330 and fully conquered by the end of the fifteenth century, amounted to a sort of prelude to the big event: the domination of the New World, which would begin a century later and which would still not be entirely finished by the time Leibniz proposes his militia for, among other things, setting up an American colony. When Leibniz proposes “an island off Africa”, such as Madagascar, this is evidently because the Canaries have already been taken. But it is the Canaries, and the expansion of the Spanish Empire out from there, that serves as Leibniz’s model and inspiration. Of course, eventually Madagascar would be decisively taken by the French, but only in the 1880s, and then on the basis of a notional claim extending back to a small French settlement that had disappeared by 1674. Leibniz’s advice was taken, but with a delay of 200 years. Leibniz is particularly adamant, in his later years, that all human beings are equally human. While even in 1671 Leibniz seems to believe that ‘semi-beasts’ are only in this state as a result of their upbringing, later Leibniz will come to find it unconscionable that any human being should be treated like a beast, no matter what his appearance or behavioral profile. Thus in his response to Locke in the *Nouveaux essais sur l’entendement humain* of 1704, Leibniz also takes up the problem of the “Orang-Outang” (which is to say, according to our nomenclature, the chimpanzee) and speculates as to what this creature might mean for our understanding of human uniqueness:

Few theologians would be bold enough right away and unconditionally to baptize an animal that has a human figure but that lacks the appearance of reason, if it were found as a baby in the wild, and a priest of the Roman Church would perhaps say conditionally, *if you are human, I baptize you*. (Leibniz A, VI, 6:234)

Here Leibniz is evidently taking a cue from Augustine, who writes similarly: "Whoever is anywhere born a man, that is, a rational mortal animal, no matter what unusual appearance ...or how peculiar in some part they are human, descended from Adam" (Augustine 1871: 117). For Leibniz, as for Augustine, morphological deviance has nothing to do with the possession of that special office of humanity, the contemplative rational soul, and this even in the case in which the morphology is so distorted as to conceal from outside observers whether the creature in question is a human or not. Leibniz continues:

[I]t would not be known if it is of the human race, and if a rational soul lodges within, and this could be the case of the *Ourang-Outang*, an ape that is outwardly so similar to a man, of which Tulpus speaks from his own experience, and whose anatomy has been published by a learned Physician. (Ibid.)

Leibniz is committed to an all-or-nothing view of the divisions between species, even if he seems more inclined to allow that the orang-utan falls on our side of the division rather than on the side of the animals. Leibniz believes, against Locke, that species are set down once and for all by God, and that there can be no overlap between them. For Leibniz, it may ultimately be impossible to know whether an orangutan possesses a rational soul or not. If it does, then it is wholly a human being, deserving of baptism and suitable for salvation, no matter how different it looks from a 'normal' human being. There is a fact of the matter as to whether an ape is a man, or not. For Leibniz, this fact of the matter has to do with what Locke would call the 'real essence' and what Leibniz sometimes calls the 'interior nature'. There is for the author of the *Nouveaux essais* a clear criterion for distinguishing men from apes, even if it may be difficult to discern in practice on which side of the dividing line a given creature falls: that criterion is rationality, which, even if it is sometimes difficult to discern, ensures that no human being can ever be a true 'semi-beast'. In view of this deep commitment to the universal equality of different human groups, Leibniz will come to believe that human diversity is something to be studied as a cultural, rather than natural phenomenon, even if cultural differences are best investigated on the model of natural history. For Leibniz, languages should be collected and studied like so many flowers in need of classification. Thus he writes to Peter the Great's war councillor, A. Huyssen, in 1703:

Among other curiosities, geography is not the least, and I find fault in the descriptions of distant countries to the extent that they do not take note of the languages of peoples, with the result that we do not at all know the relations between them, nor yet their origins. For, as nearly all of Scythia is now covered by the Russian Empire, which extends all the way to the frontiers of Tartary and of China. I wish that the languages of nations would be noted, both those nations that are subjects of the Tsar's empire, as well as neighboring peoples or those who trade with the empire's subjects. (Leibniz 1873: 51)

Repeatedly throughout the early years of the eighteenth century, Leibniz recommends to his many correspondents heading east that they collect 'samples' of languages by writing down the Our Father in each of the indigenous tongues of the Russian Empire. As he writes in an undated letter to Lubenetskii, another of the Tsar's councillors (surely composed between 1705 and 1710: "I have long wished to have *specimina linguarum* that are in the Tsar's territory, and of those bordering it, in particular I would like to have Our Father written with interlinear translations, as well as certain common words that are used in the languages. *In specie* I would like to have news of all the different sorts of Tartars, and of the distinctions between them. It is through languages that one can best distinguish peoples" (Leibniz 1873: 50). For Leibniz, we can identify another nation through the outward sign of language, but we cannot really *know* a nation, that is, know what it is like to be a member of another culture, except from within. This would seem to be worlds away from the approach of someone such as Bernier, who wishes to classify human types based on visible external markers, irrespective of the cultural identity of the members of the different types. Yet, as has been mentioned, Leibniz does offer a definition of 'race'. In notes on a text by John Wilkins taken at some point between 1677 and 1686, Leibniz lists the terms *Race*, *genus*, *Geschlecht*, and *series generationum* as synonyms, also identifying genealogy as the "explication of this series" (Leibniz A, VI, 4, 34). These definitions might be more significant than they first appear. In the earlier "Method", Leibniz had referred to the Ethiopians, Canadians, etc., variously as *gentia* and as *genera*. Does this mean that each group constitutes for him a generative unity, which is to say that each group is reproductively isolated? Certainly not: throughout his life Leibniz remains committed on Christian grounds to a monogenetic account of human origins. Even in the "Method", the barbarians are to be captured and enslaved not as subhumans, but simply as non-Christians. A generational series, then, is something quite distinct from an isolated reproductive community. Nonetheless, as biologists today know full well, interfertility *in potentia* does not necessarily lead to offspring: for many subspecies of a given species, it is enough that the one has slightly different markings on its feathers than the other, for example, in order to turn both parties off from the prospect of mating. In the *Modus instituendi*, Leibniz appears to take

Ethiopians, Canadians, and Europeans to be in much the same situation: relatively reproductively isolated because mutually uninterested, even if biologically the same, and descended from the same first ancestors. Leibniz, like Hale, is a traductionist who believes in the unity of the human species, and he also believes, very much unlike Bernier, that if there are any more fine-grained distinctions to be made, these will be made not in terms of 'race', but rather of culture.

6. Conclusion

Hale and Leibniz are very much in agreement as to the causes, and as to the relative superficiality, of the diversity and dispersion of human beings. In their most intense engagements with the problem of racial difference, Leibniz takes on Bernier, who has an explicit system of classification of different human groups, but no account of the causes of the differences between them, while Hale takes on La Peyrère, who is not terribly interested in classifying different racial types, but with his pre-Adamism provides the theoretical possibility for what would eventually emerge as a straightforwardly racist system of classification. Hale is most interested in emphasizing the unity of the human species on the basis of biogeographical explanations; Leibniz is also interested in this, but in addition wants to account for the diversity of the human species by appeal to linguistic and cultural differences. In this respect, while Leibniz has long been mistakenly placed in a historical line of thinking that leads to the physicalist account of difference, of Blumenbach and others, in fact Leibniz's true interests anticipate the interest in cultural difference of Herder and, in turn, of the 'soft' romantic nationalists of the nineteenth century. The most widespread approach to the question of human diversity in the seventeenth century sought to account for it by appeal to traduction (which has every human being descending from the same parents) coupled with degeneration (which accounts for variation in appearance despite shared descent). Where polygenesis theory is promoted in the early modern period, for the most part this appears to be motivated by an interest, strangely enough, in *appreciating* rather than devaluing non-European peoples and their accomplishments: to argue that they were separately created was a way of arguing that they had their own histories, including their own chronologies, and that these should be taken seriously even if they contradict our own. The early defenders of pre-Adamism denied traduction, though not as a way of highlighting the distance and difference of non-Europeans, but rather of highlighting the existence of parallel traditions. The remarkable malleability of the purposes to which the theory of polygenesis can be put strongly suggests that a cognitivist account of the innate basis of racial classification cannot tell us much of anything about racism as a factor in the social

and political sphere. It also suggests that if we want to understand the history of racism, we would do well to turn our focus from the various accounts that have been offered of the nature of human difference, and pay attention instead to the way in which these accounts serve to buttress or undermine the political and economic institutions – such as missions in the sixteenth and seventeenth centuries, or slavery in the nineteenth century – that benefit from systemic inequality. Nor should it be assumed that in every time and place belief in an essential difference between one's own group (such as Europeans or people of the Abrahamic faiths) on the one hand, and outsiders (such as New World natives or sub-Saharan Africans) on the other, will necessarily amount to racism in any meaningful sense.

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Scientific revolution in the moral sciences

The controversy between Samuel Pufendorf and the Lutheran theologians in the late seventeenth century

Merio Scattola

The academic discipline of natural law (*ius naturae et gentium*) is a typical product of the late seventeenth century and its history shows many similarities with the history of the natural sciences during the time of the “scientific revolution”. It was officially introduced as an academic teaching for the first time in 1663, when Karl Ludwig (1617–1680), prince elector of the Palatinate, appointed Samuel Pufendorf (1632–1694) as professor of natural law at the university of Heidelberg. This was the starting point of an important and long-lasting development in two respects. This version of natural law was soon established successfully in all German universities, beginning with the Protestant territories and further reaching the Catholic states; it remained a noteworthy part of the academic education until the end of the nineteenth century. Natural law theories soon spread to Great Britain, where they were applied to political and juridical topics, and to southern Europe in the second half of the eighteenth century. But the appointment of Pufendorf is significant also in a second sense, which is related to the question of controversy since from the very beginning of his teaching he was involved in a fierce debate about the epistemological foundations of his doctrine. This debate lasted over two decades. All Pufendorf’s antagonists were Lutheran theologians, teaching in Lund and Leipzig, who insisted on a more orthodox foundation of moral philosophy. This controversy was therefore a struggle between two important sciences or disciplines: natural law and theology, both of which pretended to provide an exclusive foundation for moral philosophy by applying two different methods. But the topic of the discussion was even broader, since natural law and theology did not only intend to show how we should understand “moral knowledge”, but they also defined such knowledge. The debate therefore corresponds to a “scientific revolution” in the moral sciences while at the same time it affords important insights into the history of controversy.

1. The controversy on the foundations of natural law

The theoretical reason for the dispute on natural law was already expressed in Pufendorf's first work, *Elements of a Universal Jurisprudence*, published in 1660, where he tried to demonstrate the consistency of a legal system built on a purely synthetic method, that is, introducing some axioms and developing from them a series of theorems (Pufendorf 1999 [1660]). Pufendorf followed here a hint of his teacher Erhard Weigel (1625–1699), who in a private manuscript, "called *Ethica Euclidea*, had already tried to show that moral doctrine could be demonstrated by means of mathematic method" (Thomasius 1719: 93); Weigel published later an *Arithmetical Description of Moral Philosophy* (Weigel 1674).

Pufendorf's *Elements* was also the reason why he was assigned the teaching of natural law in Heidelberg, thus launching the history of this modern discipline in European universities. Nevertheless, the interest in a new organisation of jurisprudence, which he manifested in his book, was actually a matter of wide discussion within the German learned culture milieu of that time. In 1663 Johann Christian von Boineburg (1622–1672), prime minister of the prince elector of Mainz, invited some leading philosophers to write for him a corpus of natural jurisprudence. He addressed his request to Christoph Forstner (1598–1667), Hermann Conring (1606–1681), Johann Heinrich Boeckler (1611–1672), and to the same Samuel Pufendorf (Thomasius 1719: 91; Palumbo 1990; Hochstrasser 2000). The participants in this correspondence had actually represented three different positions in the history of legal and political thought. Conring was the most famous advocate of political Aristotelianism; Boeckler was inclined to Neo-Stoicism and Tacitism, following the example of Iustus Lipsius (1547–1606), and emphasized the doctrines of reason of state and *arcana imperii*. Likewise, Forstner, chancellor in the Earldom of Mömpelgard, had written a long political commentary on Tacitus' *Annals* (Forstner 1662 [1628]). In contrast, Pufendorf had already formulated his project of an exact science of morals and jurisprudence in 1660 in his *Elements*. The scholars involved in the correspondence reacted to the invitation of Boineburg in four different ways: Forstner remained silent; Conring published his *Propolitica* (1663), where he expounded his theory of architectonic politics by referring to the Frisian jurist Joachim Hopper (1523–1576); Boeckler edited a commentary on Hugo Grotius (1663). Only Pufendorf accepted the invitation and implemented his program in the following years within two major works: *On the Natural Law and Law of Nations* (1934 [1672]) and *On the Duty of Man and Citizen* (1927 [1673]). Boineburg, influenced by Conring, later also invited Gottfried Wilhelm Leibniz (1646–1716) – albeit as his juridical advisor – who became in the following years a ferocious enemy of Pufendorf (Döring 2010).

In his two treatises Pufendorf applied an apparently less coherent method than he had done in the *Elements* (Hochstrasser 2000: 42–47) and tended to make use of historical proofs alongside analytic reasoning, although insisting on the same general epistemic premises expressed in his preceding treatise: that natural law should be totally independent from theology, that it proceeds only on the basis of an internal principle, and that such a principle has the form of a first proposition in a logically consistent system. This declaration of independence from theology was clearly stated in the preface to the *Duty of Man and Citizen*, where Pufendorf categorically stated:

Therefore it is manifest that from three founts, so to speak, men derive the knowledge of their duty [...]: namely the light of reason, the civil laws and the particular revelation of the divine authority. From the first flow the commonest duties of man, especially those which make him sociable with other men; from the second, the duties of man in so far as he lives subject to a particular and definite State; from the third, the duties of a man who is a Christian. From this, three separate studies arise, the first of which is the natural law, common to all nations; the second, the civil law of the single individual States [...]. The third is called moral theology [...]. Consequently, if there be some actions which we are bid by divine literature to perform or not to perform, yet whose necessity can not be grasped by reason left to itself, those actions fall outside the natural law and properly look toward moral theology. Moreover in theology law [...] has annexed a divine promise and a certain sort of pact between God and man. From this consideration the natural law abstracts [...]. Furthermore, that is by far the most important distinction whereby the end and aim of the natural law is included only in the circuit of this life, and therefore it moulds man accordingly as he ought to lead this life in society with others. But moral theology moulds a man into a Christian, who should not only have the purpose of passing honorably through this life, but who especially hopes for the fruit of piety after this life [...]. Hence the decrees of the natural law are adapted only to the human forum, which does not extend beyond this life [...]. From this also it follows that the human forum is busied with only the external actions of man, while to those which he concealed within the breast and produce no effect or sign outside it does not penetrate and consequently is not disturbed about them. Therefore, the natural law is likewise concerned to a great extent with the directing of the external actions of man.

(Pufendorf 1927 [1673]: A6r–8r)

This long passage is a plain and complete summary of the moral epistemology implied in natural law. It asserts that “human reason left alone” can find all rules of social life; that it needs in this search no external aid; that its conclusions apply only to the earthly life and that they concern only exterior actions. The first chapter of the same treatise explains what “human reason left alone” really needs in order to produce this kind of secularized knowledge: firstly a perfectly rational

being, that is a human being, and secondly a rational starting point for the rational activity of that being.

Upon these assumptions Pufendorf defended the doctrine that natural law and morality in general could not be sufficiently founded on the basis of either revelation or innate ideas, which are a kind of indirect revelation. Moreover, he saw in this ancient doctrine a theological danger to the integrity and independence of legal theory (Pufendorf 1927 [1673]:22–23; 1934 [1672]:201–205; 2002 [1686]:161–165; Thomasius 1719:93–127) and tried to explain directly the difference between the doctrine of the *notitiae inditae* and the modern natural law. We can accept, he argued, the teaching that natural law is immediately evident to all human beings. Nevertheless, this assumption does not mean that there are in our hearts some real and clear statements about the actions we have to do or to avoid. On the contrary, we should understand that old and common opinion in two ways. On the one hand, it means that we can investigate and comprehend all laws of nature with our reason; on the other hand, these laws are so clear and simple that all human beings agree about them in such a way that they cannot be forgotten once they have been understood. Only in this sense, we should expound the Holy Bible where it is said that the law of nature is written in the human heart, as in *Romans*, 2, 15: “Which show the work of the law written in their hearts, their conscience also bearing witness.” In the *Duty of Man and Citizen* Pufendorf described his position thus:

The common saying that that law is known by nature, should not be understood, it seems, as though actual and distinct propositions concerning things to be done or to be avoided were inherent in men’s minds at the hour of their birth. But it means in part that the law can be investigated by the light of reason, in part that at least the common and important provisions of the natural law are so plain and clear that they at once find assent, and grow up in our minds, so that they can never again be destroyed, no matter how the impious man, in order to still the twinges of conscience, may endeavor to blot out the consciousness of those precepts. For this reason in Scripture too the law is said to be “written in the hearts” of men. Hence, since we are imbued from childhood with a consciousness of those maxims, in accordance with our social training, and cannot remember the time when we first imbibed them, we think of this knowledge exactly as if we had had it already at birth. Everyone has the same experience with his mother tongue.

(Pufendorf 1927 [1673]:23)

The laws of nature are therefore by no means formed by a set of actual and distinct innate ideas (*actuales et distinctae propositiones*), which are present in our minds since we have been born. Indeed, our soul lacks any pre-existing notions and is empty. The contents of natural law are not given to us, but we must search for them, find them out or synthesize them. In this sense, if we could penetrate into

the mind of a newborn, we would find only a faculty of rational calculation. Using this ability, he or she can investigate all different phenomena of the moral world and identify a first truth governing them. This principle is then the starting point of the whole system of moral and legal obligations, which is produced by applying our rational capability to that first and simple truth.

Pufendorf's account became later on a common theory; Christian Thomasius and Christian Wolff expressed themselves in similar terms (Thomasius 1688: 67–69; Scattola 2001: 136; Schröder 2004: 19–23). Wolff wrote for instance that “human beings have no ideas of natural law before they start using their rational faculty” (Wolff 1971 [1738]: 206–207). The modern theory of natural law rejected the doctrine of innate ideas because they evaded rational investigation and imposed themselves immediately as unquestionable truths. While the ancient doctrine considered the human being as full of truth – the truths that God wrote in the soul during creation – the modern theory of natural law assumes that humans are fundamentally empty. Thus, only those rules that are produced by a rational argument may be subsumed into the theory of natural law. Gottfried Achenwall (1719–1772), who taught *ius naturae* in Göttingen in the middle of the eighteenth century, acknowledged as actions pertaining to natural law only those ones which derived from the “higher faculty of the soul,” which is peculiar to human beings and consists in the capability of “making notions, judgements and arguments, observing, comparing and abstracting” (Achenwall and Pütter 1750: 9, 13). Only these notions are truly deduced in a consistent way and deserve to be part of right reason. But in a human soul other forces too are present, which arise from some obscure imaginative source. Such are the instincts, which we share with the animals, and are basically different from the rational will, the true object of practical philosophy (Achenwall and Pütter 1750: 13). These obscure motions of a lower order, which do not belong to the law of nature, correspond to the innate ideas of the old tradition.

The danger implied in the new method was immediately perceived within the academic world, which reacted to the propositions formulated by Pufendorf in a resolute way (Pufendorf 2002 [1686]; Thomasius 1719: 93–127; Palladini 1978). Actually, both main points in the ensuing heated discussion were epistemological, the first concerning the doctrine of innate ideas, the second regarding the possibility of an ontologically independent morality (*perseitas moralis*).

The idea that natural law should be fully independent from moral theology was sharply criticized by Lutheran theologians firstly in Lund, where Pufendorf taught in the years 1670–1677, and then in Leipzig. The first attack came in 1673 from Josua Schwartz (1632–1709), who distributed among the students an anonymous *Selection of Some Novelties, Which Samuel Pufendorf Published in His Book On the Law of Nature and Nations against the Orthodox Faith* [Schwartz 1673]

accusing Pufendorf of being a *novator*. Since Pufendorf appealed to the King of Sweden, Schwartz's manuscript was prohibited in Lund, but after some months was published again in Giessen with notes by Nikolaus Beckmann (?–after 1678), another professor of divinity at Lund. In the same year Friedrich Gesenius (?–1687), under the pseudonym of Christianus Vigil (Gesenius 1673), accused Pufendorf of being the author of the *Discourse on Polygamy*, which had really been written by Johann Lyser using the name of Alethophilus Germanus (Lyser 1673). In 1674 Pufendorf replied to the charges of Schwartz and Beckmann with an *Apology* (Pufendorf 1674a), but meanwhile the attacks of Gesenius continued with two new pamphlets in 1674 and 1675, in which he defended the *perseitas moralis* as the true principle of natural law. Pufendorf answered in 1674 with an *Epistle to Johann Adam Scherzer* (Pufendorf 1674b), a theologian in Leipzig, thus extending the controversy to continental Germany and provoking a violent reaction. Among others, also Valentin Veltheim (1645–1700) in 1674–1675, Nikolaus Beckmann in 1677, Valentin Alberti (1635–1697) in 1678, and Johann Joachim Zentgraf (1643–1707) in 1681 partook in the polemic.

The most systematic among these authors was Alberti, who had formerly proposed a Christian natural law compatible with the Revelation and reacted now to the immanent views of Pufendorf reasserting the doctrine of innate ideas. He suggested that God had given through personal revelation a complete set of moral notions to the first human beings and that Adam in Paradise was able to act according to natural law. Therefore, natural law must assume the condition of Adam before the original sin as its true unit of measure.

The debate went on for several years and involved other theologians, above all from the Lutheran faculty in Jena, who proposed the doctrines of Hugo Grotius (1583–1645) as an alternative to Pufendorf, and consistent with the teaching of Christian orthodoxy (Scattola 2004a: 96–101). Alberti's ideal of Adamitic condition and the use of Grotius are both present in a work of Veit Ludwig von Seckendorff (1626–1692) on natural law, the *Project of a Universal or Natural Law according to the Books of Hugo Grotius* (1691), which strongly defended the traditional doctrine of *notiones inditae*. Seckendorff based his theory on the argument that God is the author of the whole universe and therefore he gave to each creature all rules necessary for its existence. In this sense, there must be an obligatory idea of natural law, which exists independently of human reason. During the creation of human beings, God impressed his will upon their hearths as a set of clear obligations, which really guided the behaviour of the human ancestors in heaven. Adam's fall destroyed the perspicuity and the influence of those ideas, but could not cancel them. God repeated then the same notions in a clear way in the Ten Commandments he gave to Moses and in the Christian message of the Holy Gospel. Thus, the Decalogue is the best summary of natural law that we have ever

possessed and it is valid not only for Jews and Christians, but holds for the whole of human kind because it declares rules present in every soul and known to some extent to everybody (Seckendorff 2006 [1691]: 405–408).

Pufendorf reacted to these arguments on several occasions and tried to settle the quarrel with a systematic exposition in his *Essay of the Controversies on Natural Law* (1678) and then in the *Gleaning of the Controversies* (1680), and finally in the *Eris Scandica, the Swedish Quarrel* (2002 [1686]). Here he shaped the whole discussion like a genuine controversy, an *eris*, and republishing his most significant contributions, he presented both his arguments and those of his opponents. In these works he reached also the clearest view about his own methodological approach and formulated the program of a rational system of moral philosophy (Pufendorf 2002 [1686]: 102–197).

Pufendorf's attempt to settle the quarrel failed and the controversy went on with new participants. Christian Thomasius (1655–1728) supported Pufendorf and published in 1688 his *Institutions of Divine Jurisprudence*, where, in the first book, he defended Pufendorf from the objections put forward by Alberti (Thomasius 1688: 1–94). On the one hand this new episode settled the quarrel concerning Pufendorf's natural law, but on the other hand it opened a new discussion between Thomasius and the Lutheran theologians, which led to the expulsion of Thomasius from Leipzig and to the foundation of a new university in Halle in 1694 (Thomasius 1719: 93–124).

We can see here, among other things, a first and general feature of controversial argumentation in early modern times. Controversies actually quite seldom came to a settlement. They were usually carried on by other actors, like in this case, where Thomasius inherited and pursued Pufendorf's controversy against the Lutheran theologians. In the present case, the continuation of the controversy was probably promoted by the fact that it had several facets, pertaining to theology, jurisprudence, and political theory. So, when the discussion on one of these subjects was exhausted, it could be resumed on another one.

2. Two eras in the history of moral doctrines

The controversy about the foundations of natural law is important in the history of the moral sciences in two regards. First, it reveals some basic features of natural law as a form of human knowledge, and therefore helps us to broaden our views on the 'history of science'; secondly, it gives us some information about the 'history of controversy' as an instrument of scientific argumentation.

As for the first point, we can observe that an important change took place in the seventeenth century within the moral sciences and that a new model of

jurisprudence and political theory prevailed over the traditional view. In order to interpret this change, we can successfully apply some of the concepts developed in the history of the natural sciences and mathematics, investigating for instance the contributions of Galileo Galilei, Isaac Newton or Robert Boyle to modern science. Among these tools are the idea of a 'scientific revolution', 'method' and the concept of 'paradigm', which permit us to write a history of jurisprudence or a history of natural law in the same sense as historians of natural science have written a history of physics. The most important move in this history of moral and political sciences is that we should not only investigate the contents or the ideological positions defended or criticized by past authors, but we should consider first of all the form of knowledge they adopted (Stolleis 1988: 43–46; Rohls 1994; Scattola 2008: 77–78). How did jurisprudence or political theory conceive itself? Which position did they occupy in the whole of human knowledge? What were their main epistemic features? Which instruments or methods did they apply to reach their aims? Following this program, we can eventually discover that a 'scientific revolution' did not happen only in physics or astronomy, since we can describe a similar transformation in moral sciences too, or in that part of human knowledge that in early modern times was called *philosophia practica*. In fact, a new paradigm, a new method of explaining human actions, and a new configuration of the fundamental epistemic features characterized the practical philosophy of the late seventeenth century, and this epistemic change had its evident expression in the foundation of a new moral science and of natural law.

In the second half of the seventeenth century such a "new political science" – as it was defined by Leo Strauss in 1935 (Strauss 2001 [1935]: 171–173; Wolf 1969: 30–66) – aimed at giving a rational, compelling, and definitive explanation of human behaviour as long as it concerned visible and external actions. In this sense, it proposed itself as the unique and true foundation of human morality. However, this purpose could be achieved only through a new method and a new way of constructing moral arguments. Therefore, the scientific revolution within the moral sciences expressed itself mainly as a revolution in the methods used in moral philosophy (Strauss 2001 [1935]: 1–192). Thus the dialectical way of reasoning, characteristic of traditional or Scholastic natural law, and which was still used in 1625 by Hugo Grotius in his *Three Books on the Law of War and Peace*, was replaced by a systematic reasoning, combining the analytic and synthetic method into the frame of the regressus, used for the first time by Thomas Hobbes in the third section of the *Philosophical Elements. About the Citizen* (Hobbes 1961 [1642]: 135–154; 1962 [1651]: 29–38) and then explicitly developed in the first section of the *Philosophical Elements. About the Body* of 1655 (Hobbes 1961 [1655]: 58–80).

Of course, all these epistemic elements – *methodus analytica seu resolutive*, *methodus synthetica seu compositiva*, *regressus* – were already well known in traditional epistemology. For example, virtually all political and legal authors of the early seventeenth century admitted that the proper method for explaining politics and civil law was the analytic one. Christian Liebenthal (1586–1647), professor of politics at the University of Giessen between 1616 and 1624, offered the following explanation in his treatise on ethical matters, clearly identifying the point of the discussion:

But if we want to achieve a point on which our mind can surely rest upon, we should conserve and strongly defend Aristotle's opinion, which stated the existence of a double order: the composing one and the resolving one. Composing is an order that begins from first and simple principles and composes them together; resolving is an order that begins with complex definitions and resolves them into their simple parts. The former one belongs only to theoretical sciences, the latter one only to practical sciences. Therefore undesirable consequences could follow if both kinds of order are confused together. Although some scholars affirm that Aristotle applied the composing order in his book *On Ethics*, as among others also Theodor Zwinger does in his foreword to his *Tables of the Nicomachean Ethics* [that is, *Prolegomena to Politics*, fo. b2r], nevertheless they all make a great mistake [...]. In fact, the same Aristotle taught with his explicit precepts and with his own example that it is most necessary to preserve with all accuracy an analytic order in practical sciences. (Liebenthal 1655 [1620]: 7)

To practical disciplines like politics or jurisprudence the synthetic or composing order could be applied only when a single topic had been detached from the entire body of a science and was then treated alone, without reference to the rest (Scattola 2002: 303–305). Late sixteenth-century logicians like Giacomo Zabarella (1533–1589) distinguished between the general order of a science and the particular order of its parts, thereby acknowledging that the whole could be treated in one way and a part in another:

But we should not omit the fact that there is a great difference between the universal and the particular order. We call "universal" that order that we use when we distribute a whole teaching, and we call "particular" that order that we apply in one part of it [...]. It could therefore happen that, in a discipline usually taught with the composing order, we use an order that is not really composing, or that in a discipline taught with a resolving order we use an order that is not resolving. (Zabarella 1578: 147)

The same distinction could be applied also to the practical sciences and lead to the conclusion that politics should be generally expounded in an analytic order, whereas some special parts of it could be also treated in a synthetic order. This opinion was held among others by Christian Liebenthal:

We should begin in every teaching from the principles [...], but in practical teachings the aim figures as principle. Consequently, this teaching of politics, like all other practical teachings, could be taught in no other way than in the resolving method [...]. Since we cannot give any demonstration and prove the contrary, we conclude that practical teachings could not be taught in other way than with the analytic method [...]. But in the meanwhile we think necessary to distinguish between the general order, which we apply in describing the whole teaching, and the particular order, which we use in describing its single parts. Treating these parts, we can surely apply the synthetic order, although the whole system of the teaching, according to its principle and aim, cannot be described in another way than with the analytic order.

(Liebenthal 1619: 16–17. Cf. Besold 1614: 30; 1625: 86–87)

On this account, the simple presence of epistemic elements, such as analytic and synthetic order within natural law, is not a sign in itself of a particular innovation. Nevertheless, a point remained always clear for the authors of the early seventeenth century: if philosophy sought to give a complete description of politics, it had necessarily to apply the analytic or resolving order, since it was impossible for this ancient tradition to use the synthetic or composing order to describe a discipline of practical philosophy.

Yet there was a fundamental difference between theoretical and practical philosophy. The former dealt with eternal and immutable objects, like numbers or stars, which also obey necessary rules (Zabarella 1578: 122–126; Arnisaeus 1636 [1615]: 289a–b; Scattola 2008: 119). When philosophy investigated these topics, it inquired into the effects necessarily produced by given causes, searching for a material implication between them. Therefore, it started with the first causes operating within a certain field of theoretical philosophy, for instance mathematics or astronomy, and then compounded them as premises within a syllogism in order to determine their effects. The result could then be used as a new premise to generate a new syllogism, and the same process was repeated continually, until the demonstration reached the most particular levels of individual phenomena. An inquiry into the topics of theoretical philosophy applied therefore an order that had three qualities: it was synthetic, deductive, and yielded necessary conclusions.

Practical teachings pertaining to human affairs like politics or jurisprudence dealt with a different kind of knowledge, and searched for the appropriate means in order to attain a certain end. The notion of the particular aim had therefore to be divided or analysed into all its parts, a process that had to be repeated until the argumentation reached the first notions or principles. In this case, the path of human knowledge came to the first causes starting from their effects. In the relation between cause and effect, that is, in the material implication, the presence of the

cause *A* implies necessarily the presence of the effect *a*, but the not the other way round. An order proceeding from effects to their causes had a higher degree of uncertainty than an order proceeding in the opposite direction. Consequently, the analytic order applied in practical philosophy could not have the same stringency as the synthetic order used in theoretical philosophy and disciplines like jurisprudence, ethics, or politics could not be defined as sciences. Henning Arnisaeus (?–1636) summarized the connection between orders of exposition, the aims of inquiry, and types of knowledge in the following way:

As a matter of fact, we search in sciences nothing else but a kind of knowledge through causes, which we obtain only by a synthetic order, whereas in practical disciplines we search for the means that permit us to achieve an end, and for this purpose we apply an analytic order. (Arnisaeus 1636 [1615]:289b)

The claim that the whole of a moral doctrine could and should be explicated with the synthetic order, a claim that was formulated by Pufendorf and his followers, was an idea that authors of the late sixteenth and early seventeenth century would have resolutely rejected. Such an assumption implied that natural law should be understood as a science characterized by necessary principles, from which a whole system of necessary conclusions could be deductively derived. More generally, this statement implied that the epistemic distinction between theoretical and practical disciplines had to be ignored and that a genuine theory was possible in both fields. However, this was not the case in ancient politics and jurisprudence.

Actually, the question implied in this account – whether politics or jurisprudence can be defined as sciences in a proper sense – was a matter of heated discussion in the traditions of practical philosophy of the seventeenth century, and the traditional answer was that politics could not be understood as a science in the Aristotelian sense (Aristotle 1934: 1139b 14–36), because it was essentially a practice of prudence endowed with some general guidelines (Scattola 2002: 288–301; 2003: 109–202; 2008: 113–124). In contrast, Hobbes, and even more radically, Pufendorf insisted that natural law was a science in the same sense as mathematics. In his *Elements of Universal Jurisprudence* Pufendorf held that natural law should be understood as a science consisting of definitions, principles, and theorems, producing a kind of knowledge about human actions with a similar degree of certainty and necessity reached by geometry (Pufendorf 1672 [1660]: *5v–6r). In this sense, the new teaching of natural law produced a change of paradigm not only in the contents of the practical sciences, by speaking of state of nature and social contract, but even more in the form of rationality it used for its demonstrations. This change of paradigm generated by Hobbes and Pufendorf in the moral sciences was parallel to the change produced by Galileo and Newton in the natural sciences.

If we look at this change, we can distinguish two historical epochs, two forms of reasoning, or two ways of producing practical knowledge. The transition, in this sense, was from a “material” reasoning to a “formal” reasoning, from ideas to procedures; this change can be viewed, as Marcelo Dascal (2005) suggests, as a passage from a soft rationality to a hard rationality. We can describe this change if we focus on the most important categories in the epistemology of moral disciplines and illustrate the changes they underwent.

The central epistemic categories were the three concepts of method, system, and principle. We have already seen that in the late sixteenth and early seventeenth centuries method and order were considered synonymous. For instance, in our last quotation from Liebethal the author used at the beginning the words “resolving *method*” and “analytic *method*” and concluded his argument by referring to “synthetic *order*” and “analytic *order*”, whereby “analytic *order*” and “analytic *method*” meant exactly the same. Logicians like Zabarella, however, separated both concepts in a clear way and argued that method is a way of finding new knowledge and consisted in the possibility of gaining unknown notions from known ideas. Method included thereafter different variations, the most famous of which was the syllogism (Zabarella 1578: 93–129; Poppi 1972: 162–195). In contrast, order was the way to gain known notions from already known ideas and hence it coincided with the skill of gathering together all pieces of knowledge we possess on a topic and displaying them in a consistent disposition, so that they could be preserved and transmitted in the best way. Order, then, was not a tool of science in a proper sense, as it did not produce any new knowledge, but it belonged to the sphere of teaching because it assured an efficient transmission of existing disciplines. In this sense, order encompassed dichotomies, tables, schemes, indexes, common places, and encyclopedic plans. Despite of this clear distinction, method and order were often confused and conflated, and this happened very frequently in practical philosophy, with the result that the “methodological” discussions about jurisprudence and politics were first of all discussions about the order their subject matter should be reduced to.

This situation was probably due to the fact that a teaching like jurisprudence did not require new knowledge but could presume that almost all its necessary notions, ideas, and cases had been already acquired. The main problem was then that all these elements were distributed in a wrong way so that their truth was not evident at all. Introducing his major legal work, the *Dicaeologica or Art of Justice* (1617), which sought to organize the whole of jurisprudence within a new scheme, Iohannes Althusius (1563–1638) formulated the following program, which represents the epistemological ideal of his time:

I have reduced to certain kinds and genres all legal topics scattered among the books of many legal authors. I took them back to their proper places and gave a right order to all those topics that had been driven away and laid far away from their own seats. In my opinion, I distributed all arguments with such an order and such a method that the foregoing topics enlighten now the subsequent ones and the subsequent ones are illuminated by the foregoing ones. Actually, some topics wandered homeless and roamed between uncertain places. Some authors allocated them to the forms of possession or of domination, others to the types of agreement, some to the offenses, some to the judgments, and some others assigned them a particular and separate place. Some other topics were proscribed from the country of jurisprudence and lived in exile, as if they were *apolides* and unworthy of citizenship in the commonwealth of law. I restored all these matters into their possessions, which they were unjustly deprived of, and took them back home and returned them to the native soil. (Althusius 1967 [1617]:2r–v)

Like other contemporary authors, Althusius identifies in this passage method and order, and makes clear that this unity of method and order is a tool that leads to the production of topological patterns. Actually, it is important to recognize all different pieces of knowledge we have inherited from the past and it is then necessary to arrange them in the best possible order so that they finally achieve fullness of meaning.

This description of the epistemic duties of a discipline is in its turn based on some important premises, which we can describe in the following way. It is clear that human knowledge is finite and that humankind has already achieved in its history all necessary experiences. All the knowledge preserved in the tradition is sufficient for all human purposes; nevertheless it has been preserved and transmitted to us in a very bad condition, so that it is now urgent to find the right way of ordering it. This would be also the better way of teaching it to the next generations. On the other hand, we should necessarily search for a common order because the different pieces of knowledge cannot be reduced to one another; nor can they be deduced from one another. If this were possible, we would solve the problem simply by finding the right starting point, from which the whole system of knowledge flows. But this was not the case in the jurisprudence and politics of the sixteenth and early seventeenth centuries, which conceived their notions, precepts, and concepts as hard elements, which could not be compressed together, because they occupied the same logical space; they rather had external borders that had to be accommodated with each other. The only possible order of items of this type is a disposition on a surface, in a way that all space is covered by them and the explanation can pass from one argument to the next in the clearest and most direct way. A disposition of this kind was called a “system” and actually coincided

with a topological arrangement within the frame of commonplaces. A system was therefore an ensemble of heterogeneous objects linked together through a dialectical order, where “dialectics” was meant as the ability of finding a point of view that made possible the coexistence of arguments that were true, but apparently contradictory. Therefore the Greek word *systema* was translated into Latin as *comprehensio*, with the meaning of “collection” (La Ramée 1577 [1543]: 11), or as *ordo constans* (Vigel 1584: 11) or as *methodica dispositio* (Hoen 1608: 2), all expressions that implied a distribution according to a frame of commonplaces.

Finally, the principles of a discipline have to be understood as the final elements identified in applying the analytic method until no more divisions are possible. These principles are the atoms of the argumentation; they cannot be further divided or reduced or unified into a single notion. Therefore they are plural and finite in number, but, on the other hand, they can be arranged together in an endless number of different combinations (Hopper 1582: 325–368). Knowledge is therefore intrinsically plural: it starts from a multitude of irreducible principles and aims at combining them together in the most consistent way. This set of combinations forms a discipline or a doctrine. In sum, if method is conceived as order, it leads to a system of commonplaces, which in turn presupposes a multiplicity of principles.

This combination of method, system, and principles formed the epistemic premises in the ancient doctrine of natural law of the sixteenth and early seventeenth centuries. The modern science of natural law elaborated by Pufendorf and defended by his followers is based on contrary epistemic assumptions: method is here a way of producing new knowledge; the system has a strict hierarchical structure, in which subsequent conclusions derive from preceding premises, and the principle is unique. The tight link between these three modern notions can be also expressed by saying that a principle is a system in its most compact or condensed form, and that a system is a principle that has been expanded in all its consequences. They are two states of aggregation of the same content, solid in one case, fluid in the other. The method is the way of transforming one into the other in both directions, a principle into a system or a system into a principle (Scattola 2004b: 3–7; 2007: 197).

The mutual implication of principle, system, and method was clearly identified by Pufendorf in his polemical writings, which in this sense offered a first complete epistemology of modern natural law. In the *Swedish Quarrel* he declared that the principle of natural law had to necessarily contain all systematic conclusions and that all rules of the discipline had to be derived from that first principle by means of an uninterrupted chain of inferences (Röd 1970: 97):

When I decided to bring natural law to the rightful form of a discipline, whose parts should be mutually consistent and derive from one another in an evident way, my first concern was to establish a solid foundation, that is a fundamental proposition, which should comprehend and summarize in itself all its precepts, from which all further rules could be derived with an easy and evident demonstration, and in which they all could then be easily resolved.

(Pufendorf 2002: 142)

Clearly, the three notions build here a kind of logical triangle. The fundamental proposition contains the whole system in itself in an implicit form, whereas the system, although here not explicitly mentioned, embraces all consequences consistently derived from the first principle. But the movement from the fundamental proposition to the system is a double one because we either go from the principle to the multitude of precepts by means of a deduction, or we return from the conclusions to the first premise reducing the multiplicity to the unity and singularity of the fundamental proposition.

In another passage from his textbook *On the Duty of Man and Citizen* Pufendorf repeated the same doctrine and asserted that all the conclusions possible within a legal system are already present in its principle and that the duty of a scientific theory consists in extracting them from the first proposition by way of deduction:

All other precepts of natural law are only consequences already contained within this general law [that is, the fundamental proposition], and all human beings can understand them with sufficient perspicuity using the natural and innate light of their reason.

(Pufendorf 1927 [1673]: 21–22)

Pufendorf's chapter containing these two last quotations was titled *On the fundamental proposition* (Pufendorf 2002 [1686]: 142) and many other authors followed his example and wrote dissertations and treatises on the question *De principio iuris naturalis*. We can mention here among others the names of Johann Nikolaus Hertius (1694), Samuel Coccejus (1699), Johann Balthasar von Wernher (1721), Daniel Friedrich Hoheisel (1731) and Johann Ulrich Röder (1783). Among the authors of the eighteenth century Nikolaus Hieronymus Gundling explained in a posthumous work the connection between principle and system in a highly precise way:

If we want to make all rightful connections within a discipline, we must first of all search for the right principle, from which we can derive all its conclusions [...]. We call first principle a fundamental proposition, on which all other conclusions depend. For instance: "Agreements must be kept, because we must live sociably". But I mean here with this notion a principle of knowing. Actually, a first principle

should be a fundamental proposition that can be known und that allows us to know, understand and explain all other propositions [...]. Secondly, such a first principle must be true, evident and adequate. It must be true, because we derive the whole truth form it. It must be evident, because we search clearness in the understanding, when a proposition is obscure [...]. Finally, it must be adequate, that is not too short and not too long, not too narrow and not too wide [...]. But one could ask: "Should we admit more than one principle?" I answer: "Johann Franz Budde has postulated three principles, but it is a contradiction in itself if you admit more than one principle". (Gundling 1744: 62–63)

This theory was defended by the most famous authors of natural law in German Protestant universities, which formed the effective core of this teaching, but it was held also by less representative catholic authors from other countries. For instance, Pietro Antonio Ghio, who taught politics in Turin in the second half of the eighteenth century, published in 1771 his *Institutions of Human Behavior Founded on the Principles of Natural and Revealed Law*, in which he developed a similar doctrine. We can now conclude that in the late seventeenth and in the eighteenth centuries philosophy and learned jurisprudence shared in continental Europe some basic perceptions of principle, system, and method, which formed an epistemic model for the moral sciences, and that this model was quite different and actually opposite to similar paradigms of the preceding centuries. The controversy on natural law between Pufendorf and the Lutheran theologians, which was the starting point of this new conception, is therefore highly significant in the history of sciences as it shows in a dramatic way a fundamental change of paradigms in the moral sciences.

The change in practical philosophy took place in the same decades as the scientific revolution. This can be a simple and casual coincidence between two historical phenomena, which followed distinct lines of evolution and only by chance underwent a significant change in the same period. Moreover, the concept of scientific revolution is itself a controversial topic in the historiography of science (Grant 1996), and the present book is obviously intended to contribute to its discussion. Nevertheless, if we accept the definition of scientific revolution introduced in the eighteenth century (d'Alembert 1751: xxvi–xxvii. Cf. Engels 1992: 991–994) and developed by Kant, we can point to a striking parallel between the natural and moral sciences. Kant described the revolution in physics as follows:

With Physics it was much longer before they took the high road of a science; for it is only about a century and a half ago, that the recommendation of the ingenious *Bacon of Verulam* partly suggested, partly, as men were already upon the track of the same, more animated this discovery, which can only be just in the same way explained by a rapid previous revolution in the mode of thinking. I will take into consideration here Physics, only so far as they are founded upon *empirical* principles.

As *Galilei* caused his balls to roll along the inclined plane, with a weight chosen by himself [...] a light rose upon* all natural Philosophers. They comprehended, that reason only perceives that which it itself produces according to its design, that it must precede with the principles of its judgment according to constant laws, and compel nature to answer its questions, and not allow itself as it were, only to be led in leading strings – for, otherwise, contingent observations, made according to no previously projected plan, are not at all connected in a necessary law, which reason yet seeks for and requires. Reason, with its principles on the one hand, according to which alone concordant phenomena could hold true as laws, and on the other, with experiment, which it has imagined according to those principles, must refer to nature certainly, in order to be instructed by it, but not in the character of a scholar, who allows himself to be taught everything which the teacher chooses, but of a constituted judge, who compels the witnesses to answer those questions which he proposes to them. And thus, in fact, physics are indebted for such an advantageous revolution in their mode of thinking only to the idea, agreeably to that which reason itself has introduced into nature, of seeking that in it (not of imagining it), which reason must learn from nature, and whereof it of itself would not know anything. By this first of all have physics been brought into the sure way of a science, when through so many ages, they had done nothing more than merely grope about here and there. (Kant 1838 [1787]: xii–xiii)

*I do not here follow exactly the thread of the experimental method, the beginnings of which are not even very well known.

What Kant presents in this passage as “a rapid revolution in the mode of thinking”, related to physics, is the same mathematization of experience that the historiography of the twentieth century acknowledged as the characterizing element of the scientific revolution (Koyré 1961 [1948]: 311–329; Cohen 1994: 59–72; 2005: 9–33). It is a matter of discussion, whether and to what extent such a revolution took place. Or whether the philosophy of the late sixteenth and early seventeenth century possessed all necessary intellectual means, and how it really used them. Kant however demonstrates in this passage that the eighteenth century was aware of a fundamental change in the history of mathematics and physics, which implied a program involving three elements. First, the study of nature had undergone a profound evolution and a new kind of knowledge had arisen. Secondly, human reason inquired into natural processes by way of experiments. Actually, in this passage Kant sketches a brief history of the Galilean or experimental method. Thirdly, scientific research determined in the natural world the existence of rational laws that were mathematically necessary. This was what was perceived and understood as “a revolution in the mode of thinking”.

The change that occurred within the moral sciences shares most of its characteristic qualities with this concept of “revolution”. It was a sudden epistemic change and it was concerned with the identification of a sphere of necessity within

human experience. Both physics and natural law tried to determine fields governed by necessary laws within human experience, which previous centuries had interpreted as a field dominated by chance and partly controllable only through a dialectical discourse. Both tried to reduce a topical complexity to a simplicity that could be foreseen and dominated. Of course, natural law did not use the experimental method to identify laws of moral necessity, but it applied a version of *recursus* consisting of an induction of the principles from experience and of a deduction of the experience from principles (Hobbes 1961 [1655]: 58–80). In this sense, the idea of a sudden and definite change – of an epistemic revolution – was formulated at an early stage in modern moral sciences, possibly earlier than in the natural sciences. Perhaps, the awareness that science had passed from the world of the “more-or-less” to the universe of precision manifested itself for the first time systematically in the teaching of natural law.

3. Two eras in the history of controversy

There is also a second reason for the importance of the controversy between Pufendorf and the Lutheran theologians and it concerns the particular issues it dealt with. Actually, the main issue in this controversy was the quality of the principles of knowledge. On the one hand, Pufendorf insisted that we can establish a first beginning for practical knowledge and that the whole system depends on it (Scattola 2001: 133–137). This first notion – for instance the idea that all human beings are sociable or that they all strive for survival – is then a product of human understanding and every human being can attain it by means of simple induction, without any external aid, that is, without divine intervention. Once we have reached such a starting point, we need logical tools or rules in order to explicate all possible conclusions implied by the first principle. However, these are nothing else than the rules of a consistently reasoning mind. Therefore, what human beings need in order to understand and regulate their behaviour on this earth lies perfectly within the power of human reason. Following the natural way of thinking, all human beings can reach both the rules that govern the human understanding and the first principle they have to start with, that is, both the transformative grammar and the first statement that provides the starting point in the process of knowledge.

This conception of natural law implies a complete secularisation of the human mind, for the world of knowledge is immune to all kinds of transcendence. Secondly, it implies that human knowledge evolves in a linear way, beginning from a first principle, and then proceeding further to all necessary conclusions.

Consequently, the idea that two or more principles can coexist and be equally true must be excluded as epistemologically contradictory. On the contrary, a true way of arguing must assume that only one point of view is absolutely true whereas all opposed positions are false. The duty of argumentation, and of course of controversy, is then to demonstrate the only true position and to exclude false ones by developing true consequences from true arguments. This excluding strategy presumes a radical alternative between rival positions. Something is or is not, and there cannot be a third (*tertium non datur*). Mediation or compromise is not admitted here, and the unity of the epistemic system is granted by the uniqueness of its principle.

So much for Pufendorf and the “modern political science”. But the opposite view of the Lutheran theologians referred exactly to a different and ancient way of arguing, which always tried to show the truth of both positions in a question. The theologians mainly opposed two arguments to the doctrine of Pufendorf, claiming firstly that human reason cannot achieve with its own forces a sufficient degree of knowledge, because it always needs some elements taken from external sources. Human reason receives, they argued, its first notions from the Almighty in the form of innate ideas. Secondly, if we possess a set of innate ideas, impressed by God onto our soul during Creation, then the first beginning of moral teaching cannot be a single principle, but appears necessarily as a plurality of notions, which should be combined together in order to develop a whole doctrine.

Having more than one principle means that an idea cannot be reduced to another idea. For instance, we cannot deduce the prohibition of theft from the prohibition of murder. All innate ideas are in this sense independent. At first, they appear as separated from one another or even contradictory to one another, like stones or bricks that cannot be compressed together. If human understanding is at first in this condition, dealing with a plurality of independent or apparently contradictory principles, its first task is to demonstrate that each principle is true, and then it has to combine together all true arguments in such a way that their truth is preserved. In other words, the first notions must be all true in the same way and there must be a relationship which can justify them all within a common argumentation.

This concept of human knowledge, defended by the Lutheran theologians, is clearly different from the one promoted by Pufendorf. Valentin Alberti insisted in his polemical writings, against Pufendorf, that natural law cannot be reduced to a single fundamental proposition, but that it had to start with a plurality of rules corresponding to the Ten Commandments and deriving from Divine Creation. He concluded that Pufendorf’s first principle was nothing more than a rule of composition that forced natural law into a wrong order.

Beside four general laws, I have adapted the number of these rules to the moral circumstances, which show in this respect a great variety of differences. Therefore, I preferred to enumerate a number of clear distinct ideas as a kind of first and universally admitted foundations rather than to comprise or, better, to confuse them all in a single proposition. The rule that Pufendorf in the same passage exalts as his own merit [*Specimen controversiarum*, 4, 22, p. 223] expresses only the idea that the whole natural law is important for human sociability and is therefore a meter that measure in an inverted order the former with the latter. (Alberti 1678: 40)

All other opponents of Pufendorf repeated this basic pattern consisting of innate ideas, Decalogue, and method of composition (Henniges 1673:7r; Alberti 1678:17; Seckendorff 1685: Additiones, 315–317; Alberti 1688:21; Simon 1688: a4v). The concept of controversy implied and practicable in both positions was therefore different. In one case, in the theological and older position, we have an including doctrine of knowledge; in the modern and philosophical point of view we have, on the contrary, an excluding doctrine. In the first case, we have a multiplicity of true principles or positions generating the system of knowledge; all opponents have therefore some part of truth that must be preserved and the argumentation has to find the way in which all true positions harmonize with each other. In the second case, only one position can be true because only one principle can generate the system of knowledge. Therefore, the argumentation must reduce all different points of view to the first principle, excluding as false all those opinions that are not consistent with it.

In this sense, the controversy between Pufendorf and the Lutheran theologians on the main features of natural law was at the same time a dispute about the epistemological foundations of modern science, especially about the possibility of defining moral sciences in a proper fashion, which later on actually developed into our social and human sciences. For these reasons, this discussion was at the same time also a controversy about the principles, foundations and rules of controversy itself. When the theologians defended a plurality of fundamental propositions, expressed in the Ten Commandments, they insisted on the idea that controversy has to do with a multiplicity of points of view and that in exchanging arguments it should combine together different positions through a dialectical process. In this sense, they worked with the tools of the tradition, e.g. by always starting from a plurality of definitions in explaining the essence of law. Valentin Veltheim, who explicitly intended to discuss “controversial questions with the traditional and easy method”, began his exposition with the three main notions of law, approved by most theologians, admitting afterwards some other secondary definitions (Veltheim 1676:8). Pufendorf on the contrary reduced the different points of view to a single principle and postulated the identity of principle and

system (Scattola 2004b: 3–7; 2006: 59–66). Therefore, he reduced the field of controversy or even intended to destroy the possibility of controversy. If our conclusions are correct, after the foundation of natural law upon a scientific basis, legal controversy in an ancient meaning, as a confrontation of contradictory points of view, became impossible. In modern moral sciences only a systematic demonstration based on a necessary principle was now admitted, whereas a controversial search for the truth present in all different positions had to be excluded.

If this hypothesis about the end of the controversy in early modern thought is valid, then the debate about natural law in the last decades of the seventeenth century represents a main change both in the history of moral doctrines and in the history of controversy, and it identifies two major eras: the era of controversy on a dialectical basis and the era of scientific argumentation on a systematic basis. The “revolution” that took place within the moral sciences can therefore be understood in two ways, regarding both the history of the sciences and the history of controversy, since a remarkable change took place both in the method applied for arranging contents and in the concept of science and knowledge in general.

With respect to the history of the sciences, we should actually question the meaning of the word “revolution” in our explanation. Did moral teachings share the same “scientific revolution” as the modern natural sciences and if so, in what sense? We can answer this question by simply observing that two main changes took place in the same years, in both fields of knowledge, which enables us to draw a clear parallel between them. In this sense, there is a historical analogy between the moral and the natural sciences. The comparison can go a step further to locate a common element beyond a mere analogical one because both fields reacted against the same previous concept of knowledge. The use of controversy was not exclusive to disciplines like jurisprudence, ethics or theology, and if we define it as a dialectical strategy, as we did above, we can easily see that also physics and medicine had in pre-modern times a “controversial” nature. Medicine was understood as a “practical discipline”, which managed to organize its aims into sub-disciplines by means of the analytic method (Arnisaeus 1636 [1615]: 2b; 289a–291a; Scattola 2008: 118). So, the “revolution” within the moral and the natural sciences evolved from the refusal of the same topical and dialectical methodology. A substantive identity between both fields, as if both “revolutions” were two expressions of the same structural change, must then be sought on a deeper level (Scattola 2006: 107–112).

We can assume that the idea of controversy underwent a similar change. Our hypothesis presumes that it was particularly productive before modern times, since it was a fundamental means of searching – that is, of understanding – knowledge, but it lost this function, at least in the moral sciences, in the middle of the seventeenth century. This meant a change of pattern in the way of conducting

(and of representing) a philosophical controversy, and at the end of the eighteenth century we can find two different paradigms at work. The debates between Lessing (1729–1781) and Goeze (1717–1786) on Christianity and natural religion still displayed all the features of the old dialectical type, but the discussion on the principles of transcendental philosophy between Schelling (1775–1854), Hegel (1770–1831), and Fichte (1762–1814) incorporated the awareness of a new, systematic paradigm.

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Index

A

Académie royale des sciences 77, 100, 189, 208, 211, 215, 218
 Ad baculum 123, 131–133, 145, 147
 Ad hominem 123–124
 Adam 215, 224–225, 229, 231, 233, 239, 242, 245, 248–249, 256
 Alkahest 95–96, 99, 152, 162
 Anthropology 223, 236, 242, 248
 Arachnologist 151, 280
 Archimedean 5, 16–17, 20, 28, 30–31
 Archimedes 16, 32, 100
 Argument 5–8, 16–18, 28–29, 35–45, 49, 53–54, 56–57, 60–67, 69–73, 82–83, 85, 101–03, 106, 114–115, 117–119, 121, 126–129, 131–133, 135–137, 141–142, 145, 154–155, 162–163, 167, 171–174, 176, 183–185, 194, 224–225, 229–230, 232, 234, 255–258, 262–264, 269–270, 279
 Argumentation 3, 5, 7, 36, 53, 61, 72–73, 120, 123, 128–132, 135–136, 140, 145, 147, 172–173, 182, 184–186, 205, 257, 260, 264, 269–271, 277–278, 281
 Aristotelian 5, 13, 15–17, 19–20, 28–29, 31–32, 36, 39–42, 53, 86, 178, 183, 211, 261
 Aristotelianism 4, 40, 42–43, 51, 178, 252
 Aristotle 15–20, 28–31, 33, 38, 43, 65–66, 100, 127, 177–178, 183, 189, 199, 231, 272
 Artery 177, 179–182, 184, 194
 Assayer *see* Galileo, The Assayer
 Astronomical 5, 35, 37, 47, 49, 53, 62–63, 86, 104, 113, 117

Astronomy 4, 8, 33, 40–41, 51, 54, 63, 71, 73, 79, 167, 226, 258, 260
 Atom 85, 121, 201, 206, 232, 264
 Atomism 51, 168, 206, 214
 Attraction 6, 81, 87, 98, 103, 105, 113, 204
 Autochthony 8, 225–226

B

Bacon, Francis 1, 116, 124, 187, 193–194, 196–197, 213, 215–216, 227, 266
 Bernier, François 8, 226–227, 236–242, 240, 246–249
 Bible 225, 229, 254
 Biology 7, 186, 189–191, 193, 201, 204, 206–207, 209–214, 216, 218, 281
 Blood 7, 88, 164, 171–172, 174–177, 179–186, 189, 192–193, 196, 202–203, 225–226, 235–236, 242
 Boantz, Victor 5–6, 77, 92, 99, 167, 272, 277
 Body 15–24, 28–29, 38, 40, 45–46, 50, 63–66, 78–79, 81–83, 86–90, 94–96, 99, 102–105, 107–115, 135, 162–166, 168, 171–172, 174, 177, 179, 181–182, 184, 189–190, 192–193, 197–208, 210, 212–214, 216, 218–219, 236, 239–240, 244, 258–259, 281
 Boerhaave, Herman 192, 194–195, 203–204, 213, 215
 Boyle, Robert 7, 47, 77–81, 88–90, 92, 95–100, 158, 164, 169, 181, 192–194, 214, 216, 258
 Brahe, Tycho 33, 40

C

Cardinal 54, 56–59, 61, 68–70, 73
 Bellarmine 54, 61
 D'Este 55–58, 70
 Cartesian 21, 81–82, 85–86, 190, 194
 Casati, Paolo 17–18, 31
 Cassirer, Ernst 38–39, 51, 120
 Cause 6, 15–16, 18–23, 28–29, 38, 81–85, 87–89, 93–96, 100–102, 104–110, 112, 114–115, 136, 159–160, 163, 179, 182, 193–194, 202, 232, 236, 260–261, 280
 Causal 18, 21, 31–32, 49–50, 83–85, 199, 201, 204, 211
 Causality 20, 82
 Centrifugal 63–64, 82, 104–105
 Centripetally 82
 Christina, Grand Duchess 39
 Church 5, 32, 53–55, 58–59, 61, 176, 188, 209, 242, 245, 279
 Chymistry 7, 77, 80, 88, 90, 98–99, 152, 162–167, 169, 201–202, 217, 280
 Circulation 7, 40, 103–107, 171, 174, 176–186, 189, 192, 197, 202–203, 211; *see also* Blood
 Clarke, Samuel 102, 113–114, 120–121
 Classification 3, 153, 157, 167, 218, 227, 240–242, 245, 247, 249
 Coagulation 78–80, 87–94, 96–98
 Cohesion 5–6, 77–78, 80, 85, 87–88, 90, 94–98, 100, 226
 Comet 5, 9, 33–39, 41–42, 43–47, 49–52, 86, 100, 103, 106
 Controversial 1, 5, 7, 14, 40, 53–54, 62–63, 66, 80, 85, 101, 173, 175, 190, 192, 210, 212, 223, 257, 266, 270–271

- Controversy 1–9, 13–14, 29–33, 38–42, 46–47, 49–51, 53–58, 69, 71, 78–81, 86, 88, 98, 101–102, 106–107, 117–121, 123–125, 127–130, 132–139, 141–147, 151–152, 166–168, 171, 174–176, 181, 183, 185, 187–192, 196, 199, 201, 205, 208, 210–211, 214–215, 217, 219, 223, 226–228, 230–231, 251–252, 256–257, 266, 268–273, 275–278, 280–282
- Copernican 39–42, 44, 53–54, 56, 58, 60–65, 67, 70–71, 173
- Copernicus, Nicolaus 33, 39–40, 54, 56, 58, 62–63, 66–67, 71–73, 187, 279
- Corpuscle 87–88, 92, 94, 97, 152, 168, 192–193, 214
- Corpuscular 78–79, 97, 94–99
- Cosmology 6, 33–34, 42, 44, 46, 86, 99, 119; *see also* Cosmos
- Cosmos 40–41
- Coxe, Daniel 164, 167–168
- D**
- Dascal, Marcelo 1–3, 9, 32, 106, 118, 121, 129, 145, 167, 188, 211, 214, 242–243, 248–249, 262, 272–3, 275, 278, 280
- Davidson, Donald 137–138, 140–141, 145
- De fontibus 159, 169; *see also* Lister
- De motu cordis 7, 172, 185–186
- Debate 18, 29, 35–36, 42–43, 77, 79–81, 85–86, 88–89, 99, 107, 123–128, 130–136, 138, 142–145, 151–152, 159, 163, 166–167, 175, 177, 187–191, 215, 227–228, 231, 251, 256, 271–272; *see also* Controversy, Disputatio, Dispute
- Delamétherie, Jean-Claude 210, 215
- Demonstration 5, 7, 15, 39, 103, 119, 128, 136, 139, 169, 172–174, 176, 179, 183–185, 260, 265, 271
- Descartes, René 14, 19–21, 23, 29, 31, 38, 51, 73, 86–87, 94, 98, 112, 114, 144, 146, 184, 189–190, 192, 194–195, 207, 213, 215, 278–279
- Dialectical 1, 3–4, 7, 13, 29, 72, 131, 140–141, 143–145, 147, 173, 183, 258, 264, 268, 270–272
- Dialectics 14, 131, 140, 143, 146, 264, 275
- Dialectification, Radical 6, 124, 142, 144
- Diderot, Denis 189, 191, 198, 203, 206–213, 215, 219, 272, 281
- Digestion 192, 201–204, 211
- Disputation 3, 33–37, 43, 46, 80, 176
- Dispute 2–6, 13–14, 30, 35, 58, 78, 101–102, 118, 129, 145, 176, 217, 233, 252, 270, 273, 277
- Drake, Stillman 5, 9, 22, 31–41, 43–46, 50–51, 56, 72
- Duclos, Samuel Cottureau 77–80, 88–93, 95–100
- Dynamic 3, 16, 20, 175, 203
- Dynamics 3, 80, 121, 136–137, 139–141, 144
- E**
- Earth 8, 21, 36, 40–41, 43, 53–54, 60, 62–66, 69, 71, 73, 82–83, 103, 108–109, 152–153, 158–160, 162–163, 165, 169, 177, 193, 200, 202, 209, 224, 226, 228, 230–232, 235–236, 238, 240, 242, 268, 280
- Empirical 4–7, 35, 40, 42, 45, 80, 87, 115, 117, 129, 143, 159, 167, 184, 266
- Empiricism 41, 43, 46, 49, 99, 121, 147, 214, 216, 218–219, 278, 281
- Empiricist 197
- Encyclopédie 190, 198, 199, 204, 206–208, 214, 215, 219, 272, 281
- Enlightenment 1, 189, 191, 198, 206, 210, 212–213, 215–217, 219, 229, 273, 277
- Epistemological 6–8, 46, 49, 54, 81, 83, 93, 98–99, 114, 116, 120, 132, 134, 184, 189, 197, 218, 251, 255, 262, 270
- Epistemology 42, 44, 53, 71, 253, 259, 262, 264, 278, 280
- Ether 81, 94
- Euclidean 44, 252
- Evidence 7, 45, 53, 57, 59–60, 62, 65, 67–68, 105, 116–118, 138, 159, 165, 172–173, 182–184, 230, 232–233, 235, 238
- Experiment 3, 26, 29, 47, 54, 65, 72, 78–79, 88, 90, 100, 109, 115–118, 124–125, 127–129, 136139, 146, 152, 160, 162, 171–172, 174–176, 178–179, 183–185, 189, 192, 194, 197, 213, 219, 267
- Experimental 2, 13, 24, 28–29, 31, 80, 89, 100, 114–117, 120, 128, 130, 132, 139, 144, 146, 154, 171, 173, 176, 178–179, 183–185, 189, 192–193, 198, 216, 218, 267–268
- F**
- Fabri, Honoré 4, 13–32, 239, 279
- Fair-mindedness principle *see* Galileo
- Fermentation 87, 96, 191–192, 195, 201–205, 214, 219
- Fluid 79, 82, 88, 92, 95, 104, 106, 164, 195, 209, 214, 264
- Fluidity 77–78, 88, 92, 94–96
- Force 6–7, 18–20, 58, 63, 78, 85, 87, 96, 103–105, 109–111, 117–118, 121, 140, 181, 187, 191, 195–196, 199–200, 212, 244, 255, 269
- Form 3, 7, 19, 82–83, 94, 107, 109, 124–125, 129, 133, 138, 141, 151–153, 155, 158, 162–163, 167–168, 173, 177, 182, 189–191, 193, 198, 200, 206, 208, 211, 229, 236, 253, 257–258, 261–266, 269, 273, 277
- Substantial 15, 18, 80, 82, 91, 98, 162
- Fossil 7, 151–163, 165–169
- Freudenthal, Gideon 13, 32, 107, 121, 192, 215

G

- Galen 171, 175, 176, 179–180
 Galilean 4–5, 14, 20, 29–30, 54, 57–60, 67, 69–70, 267
 Galilei, Galileo 3–5, 9, 13–18, 20–21, 24–47, 34, 49–73, 112, 126, 147, 193–194, 198, 213, 258, 261, 278–279
 Affair 5, 51, 53, 69, 72
 Fair-mindedness principle 5, 53, 59, 61, 63, 67, 71
 The Assayer 5, 33, 35, 38–43, 45–46, 50
 Trial 68
 Galileo Affair *see* Galileo
 Gassendi, Pierre 14, 20, 29, 32, 94, 100, 188–189, 201, 211, 213, 216, 239–240, 248
 Geology 152–155, 167–169
 Geometry 14, 25, 45, 64, 79, 138–139, 261
 Glisson, Francis 190, 196, 211, 216
 God 21, 87, 98, 104, 107–108, 111–113, 117, 119, 121, 158, 162, 179, 225, 231–232, 238, 245, 248, 253, 255–256, 269; *see also* Bible, Church, Religion, Theology
 Grassi, Horatio 5, 9, 33–39, 42–46, 49, 51
 Gravity 4–6, 16–19, 21, 28, 30, 64, 77, 79–83, 85, 87, 93–94, 96, 98, 101–106, 108–115, 117–118, 120, 204, 280
 Grew, Nehemiah 40, 86, 156, 164–165, 167–168
 Guiducci, Mario 9, 35–38, 41, 51

H

- Hale, Matthew 8, 226–227, 231–235, 247–248
 Harvey, William 7, 171–188, 190, 192, 197, 208, 211, 216, 218
 Heart 88, 171–172, 176–179, 181–182, 188, 194, 202–203, 205, 254
 Heaven 19–20, 36, 40–42, 86, 106, 109, 162–164, 178, 238, 256

- Heavenly 33, 36, 40, 45, 63, 86–87, 111, 114–115
 Heaviness 17, 30, 32, 81; *see also* Pesanteur
 Heavy 16–22, 30, 32, 42, 65, 81, 83, 94–95, 103, 117
 Heliocentric 41–42, 62
 Hevelius, Johannes 47–49, 51
 Hobbes, Thomas 112, 258, 261, 268, 273, 275
 Hooke, Robert 47, 49, 51, 124, 132–133, 139, 145, 151, 154–155, 165, 279–280
 Huygens, Christiaan 14, 79–85, 88, 93–94, 97–101, 103, 105, 124
 Hydrostatics 16, 20, 29

I

- Idea 21, 33, 61, 111, 151, 173, 175, 177–179, 200, 202, 204–205, 228, 236, 255–256, 258, 261, 267–271
 Innate 92, 110, 193, 247, 254–256, 265, 269–270
 Impetus 15, 18, 20–24, 27–29, 31–32, 65, 115, 243, 279
 Inquisition 5, 32, 53–55, 57, 59, 61–62, 68, 72–73, 279; *see also* Galileo Affair
 Instrument 5, 36, 40, 43–47, 49–52, 99, 152, 164, 179, 195, 203, 206–207, 216, 219, 256–257, 278, 281
 Instrumentalism 42, 46–47
 Intelligibility 79, 82, 93, 98, 117–119

J

- Jesuit 4–5, 13–14, 17–18, 21, 28–33, 36–37, 40–44, 46–47, 124–125, 127–128, 230, 279
 Jupiter 40, 43, 45, 105, 165
 Jurisprudence 251–252, 257–263, 266, 271

K

- Kant, Immanuel 1, 8, 174, 186, 217, 226, 234, 237, 266–267, 273

- Kepler, Johannes 2, 9, 36–37, 41, 44, 49–51, 103–104, 106–107, 121, 278–279
 Kinematic 82
 Koestler, Arthur 5, 9, 58, 60, 67, 69–70, 73
 Koyré, Alexandre 1, 5, 9, 39, 51, 102, 105, 107, 108, 111, 116, 117, 119, 121, 187, 217, 267, 273

L

- La Mettrie, Julien Offray de 209, 217, 219, 281
 La Peyrère, Isaac 8, 224, 225, 226–231, 247, 249
 Law 13, 15, 20–21, 23–29, 31–32, 36, 55, 103–104, 106, 108–111, 115, 117, 120, 199, 229, 232, 238, 251–261, 263–271, 273–274, 277, 280
 Natural 8, 251–258, 260–261, 264–266, 268–271, 273–274, 280; *see also* Natural
 Of free fall 32
 Of odd numbers 15
 Le Cazre, Pierre 14, 18, 30, 32
 Leibniz, Gottfried Wilhelm 6, 8, 21, 32, 101–109, 111–121, 191–192, 194, 197–201, 203, 207, 214–215, 217, 226–227, 234, 240–249, 252, 273, 278, 280–281
 Leibnizian 29, 200
 Leibnizianism 198
 Lémery, Nicolas 165, 166, 168
 Levity 16–18, 20, 28, 30
 Life 7–8, 85, 87, 99, 145, 151, 155, 164, 168–169, 171, 177, 179, 186–192, 194–199, 201, 203–212, 214–218, 228, 232, 242, 246, 248–249, 253, 281
 Light 4–6, 17–19, 30, 36–38, 41, 49–51, 62–63, 87, 100, 107, 120–121, 123–124, 127, 138, 143–144, 146–147, 173, 244, 248, 253–254, 265, 267, 282
 Lightness 30, 32
 Lister, Martin 7, 151, 151–170, 280

Lucas, Anthony, 124–125, 128–129, 132–133, 136, 139, 141–142, 145, 147

M

Machine 32, 189–190, 195–196, 198–199, 204, 210–215, 219, 279, 281
 Malpighi, Marcello 181, 184, 190, 192
 Mariotte, Edme 79–85, 88, 94, 98–99
 Mathematical 2–5, 13, 15, 21–22, 24, 27–30, 39, 41–42, 44–45, 49, 51–52, 64–65, 72, 79, 86, 100–101, 103, 109, 111, 114, 118–119, 121, 138–139, 146, 171, 185, 188, 194, 198, 206, 214, 216, 279
 Mathematician 13, 17, 29–30, 33, 68, 80, 138, 189, 213, 279
 Mathematics 35, 39, 43, 51, 79, 138, 204, 258, 260–261, 267
 Matter 3, 6, 14, 30, 35, 41, 70, 78–87, 89–90, 93–96, 98–100, 103, 105–106, 108–110, 112–113, 116–117, 129, 133, 157–160, 165–167, 175, 181–182, 189–192, 200–201, 203–204, 206–208, 211, 213–217, 239, 244–245, 252, 254, 261–262, 267, 273, 277, 280
 Subtle 19, 81–82, 84, 94, 106
 Theory of 28, 80, 96, 99–100, 196, 201, 214
 Mechanical philosophy 77, 84, 98–100, 198
 Mechanics 4, 31–32, 80, 105, 120, 192, 210, 219, 279
 Mersenne, Marin 14, 29, 32, 230
 Metallogenesis 158, 166
 Metaphysical 6, 85–86, 102, 106–107, 111–115, 117, 120, 190–191, 199, 203, 206, 208, 210, 242
 Metaphysics 29, 72, 80, 85, 113, 115, 183, 196, 198–199, 208, 219, 242, 281
 Methodological 2–3, 5–6, 54, 103, 107, 116–118, 124, 127–134, 139–140, 142–144, 198, 206, 257, 262

Methodology 3–4, 6, 114, 123–132, 134, 136–137, 141–144, 271
 Micrographia 47, 49, 51, 165; *see also* Hooke
 Mineral 95, 151–152, 155, 158–160, 163, 196, 200–202
 Miracle 107–109, 112–113
 Montpellier 153–154, 205, 210, 219
 Moon 33, 36, 40, 43, 45, 63, 103, 109, 280
 Motion 6, 13–16, 18–24, 27–28, 30, 32–33, 36–38, 41, 54, 60, 63–65, 69, 71, 73, 81–85, 91–92, 94–99, 101–108, 110, 112, 114–115, 117–118, 172, 176–177, 191, 193, 195, 202–205, 209, 214, 219
 Circular 64, 84, 104, 177
 Natural 15–16, 18–21, 28
 Rectilinear 84
 Movement 17–18, 24, 83, 91, 103–106, 152, 171, 175, 265; *see also* Motion
 Müller, Adolph 5, 9, 57–60, 67–69, 73

N

Natural
 History 88, 92, 99, 152–153, 163–164, 167, 169, 188, 190–191, 206, 210, 236, 245, 281
 Law *see* Law, Natural
 Philosophy 79, 85, 100, 110, 113, 115, 117, 120–121, 130, 145–146, 152–153, 169, 187–188, 191–193, 197, 202, 204, 214, 223, 231, 272, 280
 Newton, Isaac 1–3, 6, 83, 85–88, 90, 95–96, 98–121, 123–133, 136, 138–139, 141–147, 168, 187, 204, 211, 258, 261, 279–280; *see also* Opticks, Principia Mathematica
 Newtonian 3, 29, 31, 85, 100, 119–121, 127, 130, 189, 203, 216
 Newtonianism 7, 277
 Norm 69, 128, 130
 Numbers 20, 23–30, 39, 72–73, 90, 97, 128, 130, 136, 153,

156–57, 194–195, 215, 230, 236, 249, 260, 264, 270
 Law of odd 15

O

Observation 43, 45–47, 49–50, 53, 120, 163, 167, 171, 174–176, 178, 181, 197, 208, 238, 278
 Occult 6, 81, 83–84, 98, 100, 105, 108–113, 117
 Oldenburg, Henry 127, 138, 155, 169
 Ontological 86, 96, 99, 190–191, 197, 199, 204, 206, 208–212
 Ontology 49, 96, 99, 193, 195, 206, 210
 Optical 5, 36–37, 44, 49, 51–52, 123, 125, 130, 144–146, 282
 Opticks 2, 85, 96, 100–101, 107, 109, 121, 136, 138–139, 145–146; *see also* Newton
 Optics 44, 49, 51, 125, 147, 278–279, 281; *see also* Opticks
 Organism 7–8, 153, 191, 198–201, 204, 207–208, 212, 216–218, 281

P

Paracelsian 89–90, 159, 164–165, 168, 201; *see also* Tria Prima
 Paracelsus 163–164, 169, 227
 Parallax 5, 35–38, 40–43, 46
 Pastor, Ludwig von 5, 57–58, 60, 67–69, 73
 Perrault, Claude 79–80, 82, 88, 94, 100, 215
 Persuasion 7, 39, 143, 174, 277
 Pesanteur 77, 79, 81–83, 94, 100
 Philosophical Transactions 6, 123–124, 146, 156–158, 162, 164, 168–169
 Physiological 77–78, 176, 192, 195–196, 198, 201, 205, 212, 217
 Physiology 4, 164, 171, 178–179, 185, 188, 190, 192, 204–205, 207, 209–211, 214, 216, 218
 Planet 36–37, 40–44, 62–63, 86–87, 103–109, 113, 165–166, 280
 Plot, Robert 7, 151–152, 162–169
 Pre-Adamism 8, 224–227, 229–230, 247

- Polygenesis 224–225, 227–231, 233, 238–239, 242, 247
- Pope
 Leo XI 57
 Urban VIII 55, 68
- Principia Mathematica 6;
see also Newton
- Proof 2, 7, 25, 65, 81–82, 106, 115, 118, 129, 131–132, 172–173, 175–176, 183–185; *see also* Demonstration
- Ptolemy 33, 40, 58, 66, 73
- Pufendorf, Samuel 8, 251–252, 255, 257, 261, 264–266, 268–270, 273–274
- Q**
- Quadrant 47
- Qualitative 18, 28, 42, 195, 203
- Quality 15, 47, 49, 81–84, 91, 108, 110–112, 139, 183, 268; *see also* Occult
- Quantitative 7, 37, 171, 179, 186, 195, 203–204, 211
- Quantity 21, 87, 103, 109, 166, 171, 179, 181, 183–184
- Querenghi, Antonio 5, 55–60, 65, 67–68, 70–73
- R**
- Race 8, 223, 231, 235–237, 240–243, 245–248
- Racism 247–248
- Rational 5–6, 86, 117, 119, 136, 141, 179, 184, 245, 253–255, 257–258, 267
- Rationality 2, 119–120, 135, 138, 141, 145, 212, 228, 245, 261–262, 278
- Ray, John 151, 153–157, 168
- Refutation 53, 60, 62–66, 68, 146, 153, 176
- Religion 33, 43, 72, 238, 240, 242, 272, 279
- Revolution 1–9, 30, 39, 52–53, 66, 72–73, 80, 98–100, 123, 125, 134–135, 137, 143, 167, 171, 173, 178, 183, 185, 187–189, 191–194, 196–198, 207, 210–214, 216–217, 238, 248, 251, 258, 266–268, 271–273, 277
- Copernican 53
- Scientific 1–9, 30, 39, 53, 80, 98–99, 123, 134–135, 137, 143, 167, 173, 183, 187–189, 191–194, 196–197, 210–214, 217, 251, 258, 266–267, 271–272
- Roberval, Gilles Personne de 14, 29, 79–85, 98–99, 108–109, 111
- Royal Society 7, 47, 100, 124, 128, 138, 143, 151–152, 154–157, 162, 164, 167–170, 178, 189, 243, 249, 280
- S**
- Scholastic 15, 28, 39, 41–42, 81–82, 105, 110–111, 117, 258
- Seed 7, 87, 90, 151–153, 158–160, 163, 165–167, 197, 199, 201–202, 209, 211, 232; *see also* Semina
- Semina 87, 152, 188, 201
- Sextant 47
- Skeptic 49
- Soul 189, 191, 199, 203–207, 210–211, 215, 245, 254–255, 257, 269
- Space 23–24, 27, 29–30, 82, 92, 95, 121, 132, 208, 263
- Sphere 18, 38, 41–42, 46, 86, 112, 135–136, 140, 142, 144, 229, 248, 262, 267
- Spirit 66, 71, 73, 83, 87, 152, 155–156, 159, 162, 165, 202–203, 241
- Stahl, Georg Ernst 191–192, 195, 199–201, 203–208, 210–211, 214–215, 218
- Star 35–37, 40, 42–44, 46, 86, 104, 107, 156–158, 163, 165–166, 168–169, 232, 260
- Steno, Nicholas 152–153, 155, 168–169, 213
- T**
- Telescope 36, 40, 43–46, 47–52, 62–64, 124
- Theology 2–4, 6, 55, 86, 102, 114, 120, 209, 227, 251, 253–254, 255, 257, 270–271, 279
- Traduction 8, 225, 230–231, 233–235, 247
- Transmutation 90, 93, 159, 165, 167
- Tria prima 89, 159; *see also* Paracelsus, Paracelsian
- Trial 53, 57, 61, 175; *see also* Galileo
- Torricelli, Evangelista 14, 16, 31
- Tychonic 41–43
- V**
- Valve 153–154, 181–182
- Van Helmont, Johannes 7, 89–90, 96, 151–152, 158–160, 162–166, 169–170, 190, 215
- Vein 81, 180–182, 184, 194, 208, 237
- Venus 40, 43, 62–63, 73, 165
- Vision 5, 9, 37, 45, 49–50, 73, 82, 282
- Vital 98, 164, 190–194, 197–199, 201, 203–204, 206, 209–212; *see also* Life
- Vitalism 4, 80, 87, 204, 209, 215, 217, 219, 281
- Vortex 82, 86, 103, 105–107, 120
- W**
- Willis, Thomas 172, 186, 195, 202–203, 213, 219

